Natural Science

A Monthly Review of Scientific Progress

APRIL 1899

NOTES AND COMMENTS.

An Ancient Mollusc.

THE genus *Pleurotomaria* is the oldest among Gastropod Molluscs, and begins its career in the oldest fossiliferous strata, the Lower Cambrian, with the earliest known Trilobites and Brachiopods.

In geological times its race was abundant, more than 1150 species having been described, and it attained its maximum, about 370 species, in Jurassic days, since which period it has steadily declined in numbers.

The genus was long held to be extinct, but in 1855 the first recent example was obtained from a great depth between the islands of Marie Galante and Dominica in the West Indies by Commandant Beau. Unfortunately this shell appears to have been tenanted by a hermit-crab, so that the animal still remained to be discovered, and it was not till 1871 that the living occupant was obtained by A. Agassiz when on the "Hassler" expedition. Since then other examples of the genus have been obtained from the West Indian region and off Japan, that, with odd specimens of unknown locality, bring the total number of known specimens up to twenty-one belonging to four species. In only six instances, however, has the animal been preserved, and five of these have been procured on American dredging expeditions.

Dr. Dall has published a few details concerning the animals of two, *P. quoyana* and *P. adansoniana* (*Bull. Mus. Comp. Zoöl. Harvard*, vol. xviii. 1889), and another specimen of *P. quoyana* has lately been entrusted by Prof. A. Agassiz to Messrs. Bouvier and H. Fischer to make as thorough examination as its preservation permitted of the soft parts. The result of their investigations now lies before us (*Arch. Zool. expér.*, Sér. iii. tome vi. pp. 115-180, pls. x.-xiii.). The animal in question had been greatly damaged in extraction from the shell; the whole of the spiral visceral-hump, with the gills and a great part of the mantle, were wanting. Under these circumstances it is hardly remarkable that the authors' observations are mainly confined to two points—the radula and the nervous system.

The radula is remarkable for the number and variety of the teeth in each transverse row, and the curvature of these rows. On either side of the median tooth in each row 117 teeth, and the rudiments of

yet another, were observed. They vary from broad, flat, simple plates, without any cusp, to narrow, sickle-shaped teeth, armed with three cusps. At the 50th tooth from the centre a tiny brush of hairs makes its appearance, which in the succeeding teeth is more and more developed, the cusps decreasing at the same time till in the 109th the brush alone surmounts a slender and nearly straight tooth. These brush-teeth seem peculiar to the genus.

The nervous system of *Pleurotomaria* is far too complicated to be treated of in the limits of this notice even in abstract, and it must suffice to say that our authors believe, and give good grounds for their belief, that in *Pleurotomaria* the clue will be found to the relationship of the nervous system of the Amphineura to that of the Gastropoda, the pedal cords of the Rhipidoglossa originating by the fusion of the pedal and pleural cords of the Amphineura, as evinced, amongst other things, by the presence of grooves along the under surface of these "pedal" cords in *Pleurotomaria*.

It is an open secret that a more perfectly preserved animal of *P. beyrichii*, from Japan, is at present being investigated by Mr. Martin F. Woodward, so that further interesting details concerning the genus may be expected.

Bats and Black Grouse of Ireland.

The current (February) number of the *Irish Naturalist* contains two papers of more than ordinary interest—the first on the Natural History of Irish Bats, by Dr. Alcock, and the second on the introduction of the Black Grouse, by Mr. Barrett-Hamilton. With regard to bats, the present communication is only the first of a series of papers, and deals chiefly with the general characteristics of the order Chiroptera, with a few notes on the habits of particular species. The detailed history of the Irish forms will follow in due course. In the meantime we may express regret that the author (as we gather from a note on page 30) does not apparently intend to follow modern views of nomenclature. Personally we detest such changes, but as they are made by the advanced zoologists, the only sensible thing to do is to follow the lead. In starting with a misprint (*Atalaphe* for *Atalapha*), the author scarcely does himself justice.

Mr. Hamilton, who has previously contributed interesting information with regard to the introduction of the brown hare into Ireland, accepts the evidence of a fossil bone as entitling the black grouse to be regarded as an indigenous Hibernian bird. He then discusses the records of its alleged early occurrence in the island, and follows on with an account of the numerous later attempts to rehabilitate the species. Unfortunately, these efforts have not, thus far, been attended with the success they merit.

Bats in Burmese Caves.

THE *Administration Report of the Marine Survey of India* for the official year 1897-98 contains much valuable matter, that contributed by the Surgeon-naturalist, Captain A. R. S. Anderson, being of chief interest to our readers. We extract an account of a visit to some caves at Hpagat, 26 miles up the Salween from Moulmein :—

"These natural caves are hollowed out in the base of an isolated limestone hill, about 250 feet high, rising very precipitously from the river, and appear to be very extensive, but difficult and most unpleasant to explore, owing to the great deposits of offensive bats' dung collected into heaps on the floor by the lessee of the caves. The entrance is about 12 feet high, much ornamented by Buddhistic sculptures, and at an elevation of some 20 feet above the level of the river. As the sun was setting we took our stand on a sand-spit facing the entrance of the caves, and soon saw a pair of falcons leave their perch on the trees on the summit of the cave hill, and restlessly fly to and fro over the river. They were speedily joined by Brahminy and common kites and jungle crows, and the entire flock, to the number of probably 60 to 100 individuals, then flew to the entrance of the caves, close to which they remained wheeling about in mid-air. A few minutes later the bats began to issue in ones and twos, and were at once pursued by the watchful kites and crows, but appeared to have no great difficulty in eluding capture by their rapid and jerky flight, and their pursuers made no very determined or long-sustained efforts to capture them, but soon returned to their vigil over the cave. A minute or two passed, and a sudden rush of wings is heard, and the bats are seen to emerge from the cave in a dense stream which slowly becomes more and more closely packed, continues of about the same density for some ten minutes, and then gradually thins away, till about twenty minutes from the exit of the first bat the last has emerged. The stream of bats when at its maximum is some 10 feet wide by 10 feet deep, and so dense as to closely resemble smoke pouring from a chimney in a gale of wind, a resemblance increased by the slightly sinuous course pursued by the bats as they fly off into the after-glow. Indeed, in the great rush the bats are so crowded together that they frequently upset each other, and fall helplessly into the river below, whence by using their wings as paddles, and flapping over the surface of the water, they struggle ashore only instantly to fall a prey to the expectant crow. When the great rush occurs, the falcons, kites, and crows enter the stream of bats, and flying along, in, and with it, and striking right and left, seize as many bats as they require for food. By merely throwing my walking-stick twice into the stream of bats I obtained six specimens, but after the second throw the bats flew at a greater height than before, and out of easy reach of a stick. All the bats I obtained proved to

be *Nyctinomus plicatus*. After the departure of these small bats a much larger species emerged from the cave in small numbers. It was then, however, too dark to see even to shoot them. During the last twenty years the bats appear to have considerably diminished owing to the depredations of their bird enemies, according to the lessee of the caves, but more probably owing to their continual disturbance by the collectors of the bats' dung."

Lack of Discrimination in a Mollusc.

THE Gastropod mollusc *Xenophora* owes its name to its habit of coating the exterior of its shell with various foreign bodies. Some writers have even credited it with considerable power of discrimination in the selection of suitable materials. This idea was criticised in our review of Mr. Cooke's chapters on Mollusca in the "Cambridge Natural History," and we find our comments amply justified by Captain Anderson's observations on *Xenophora pallidula* published in the *Report* we have just quoted from. He writes: "All the examples obtained by me seem to show that the sole selective ability it displays is in rejecting foreign bodies that are too large for it to move easily, its criterion being size and not shape or material. Xenophoras from different stations are seen to be very variously ornamented, but the prevailing ornament on their backs is also that suitable-sized body of which the largest quantity is present in the same haul of the trawl, whether this be dead shells or pieces of stone, etc. At one station off Madras, in the working season of 1894, the trawl was lowered over the steamer track in 107 fathoms and the Xenophoras there obtained were found to be adorned with small pieces of cinders and coal. Here they had merely attached to themselves the commonest convenient-sized objects within their reach quite irrespective of material."

Hermit-Crab and Sea-Anemone.

A GOVERNMENT Blue-book such as this *Report of the Marine Survey of India* is so little likely to fall into the hands of naturalists, that we make no apology for giving yet another extract. This relates to a hermit-crab, a species of *Parapagurus*, apparently new to the Indian fauna, and its associate, a colonial sea-anemone named *Epizoanthus*.

"The smallest of these Pagurids inhabited gastropod shells on the backs of which were growing very small colonies of the *Epizoanthus*; but as the Pagurids increase in size while the size of the mollusc shell remains stationary, they can insert an ever-diminishing part of their abdomens in their hosts' shells; and were it not that the *Epizoanthus*

increases in size *pari passu* with the growth of the Pagurid, and grows out as a projecting rim far beyond the mouth of the gastropod, the Pagurid would speedily be shelterless. As the *Epizoanthus* increases in size, the gastropod shell, which at first occupied the centre of and supported the colony, assumes a position near the periphery of the colony and at the same time loses its lime salts so completely that it causes no grating on being cut with a knife. In the largest colonies, which are some four inches in diameter and one inch in thickness, only the very apex of the mollusc may be visible, and that only on carefully hunting for it, so deeply in the substance of the colony is the gastropod imbedded. The hollow space in the *Epizoanthus* colony occupied by the Pagurid is lined by a thin but firm layer of brown chitin, which is easily detachable from the underlying cartilaginous-like body substance of the *Epizoanthus*. The colours of the *Epizoanthus* contrasted strongly with those of its guest; for, while the latter had yellowish-pink claws, orange legs, bluish orange carapace with pale blue spots and an abdomen dirty brown in the male, crimson in the female, the ground colour of the former was madder brown with purple polyps."

We have by no means exhausted the interesting matter contained in Captain Anderson's report, to which we would refer all students of bionomics and of geographical distribution. It is a pity that the price and publishers are nowhere stated on it; but we presume that it is published in the technical sense.

The Zola Dossier.

"JE te répète que tout y est," said Dr. Pascal, as he showed Clotilde the famous family papers. And certainly we can echo this remark upon reading the somewhat bewildering pamphlet, "Emile Zola, a Study of his Personality, with Illustrations," which Mr. Arthur MacDonald, the author of "Abnormal Man," "Le Criminel-Type," "Criminology," and other learned books, has just produced. Assuredly "everything" is here,—everything that we do not want. We learn all about Zola's stomach, the size of his ears, and the length of his nose; the secrets of his digestive apparatus are laid bare to us; we learn with emotion that his sense of smell is so good that he knows beforehand what there is for dinner; "he can distinguish tomatoes, chicken, mutton, and different *species* of fish" (the italics are ours); it further appears that the "Bizygomatic diameter of head" is "146 mm.," while as to Zola's eyelids how instructive it is to note: "Palpebral fissure or slit medium; superior left one uncovered."

Against such advantages as these must be set the fact that "the teeth are bad. The alveolar arch is not normal"; while "he is exceedingly sensitive to pain."

Zola suffered much from poverty in his youth, but "when he began to be successful, he was more at ease." Quite so:

> La Palisse eut peu de bien
> Pour soutenir sa naissance;
> Mais il ne manqua di rien,
> Dès qu'il fut dans l'abondance.

There is a certain stateliness in the following splendid marshalling of diseases: "Zola has orbicular contraction, cardiac spasms, thoracic cramps, false angina pectoris, sensory hyperaesthesia, obsessions, and impulsive ideas." With this swan-like song the booklet crackles out, and we are not sorry. We do not know how much of this precious sort of nonsense is produced annually in the United States, but if by any chance anybody there or in this country is under the impression that this is science, the sooner his mind is disabused of the illusion the better. A study of Zola's personality forsooth, why, it is merely a collection of futile and rather disgusting details about his person,—a dull sort of drivel which is in reality a mingling of the impertinences of the interviewer with the indiscretions of the family physician. We cannot penetrate into the mysterious recesses of the artist's personality by observing the effect which aperient medicine produces upon him; we may measure with exactitude the length of his ears, and count minutely the number of hairs which grow on the back of his hand, but these things will not teach us wherein lies the magic of his style, nor give us the divining rod by which to discover the hidden springs of his fancy.

What is Life?

"WHAT is Life?" asks Dr. Gustav Mann (*Trans. Oxford Univ. Junior Scientific Club*, No. 6, Feb. 1899, pp. 99-101), and one seeks in his three lively pages for an answer, which, it is almost needless to say, one does not find. Dr. Mann is evidently unwilling to postulate a "special unexplainable force," but he admits that the origin of organisms meant a new synthesis. "We must have, in addition to the old laws which govern the inorganic world, new ones which govern and regulate organic existence." This may be a very sound conclusion, but it is couched in strangely mediaeval phraseology for the Oxford University Junior Scientific Club.

"The difference between inorganic and organic existence amounts to this: that the inorganic world has not the power of producing a chain of chemical events, or rather a cycle of events; but that when one chemical affinity is satisfied there ensues rest till a new substance, for which it has a greater affinity, is brought into contact with it; this meeting being one of chance (mathematical probability), or design (chemist). Organic individuals, within physiological limits, are inde-

pendent of chance, as the combination of compounds peculiar to each individual leads to the formation of a new environment consisting of complex carbon compounds, which in their turn, by acting on the world at large, so modify the latter as to make it directly assimilable by the nucleus. The nucleus in its turn forms the organic environment or cell-plasm by which it is kept in existence."

Dr. Mann calls this an "interpretation of life," but it seems to us rather dull. It is a description, and not a very lucid one, of one of the characteristics of the chemical changes observed in organisms, rather than a re-statement in simpler terms, which is the only scientific interpretation we know of. But some of Dr. Mann's paragraphs are more luminous. "In a cell we have a great many comparatively simple compounds, which only collectively form the protoplasm. What constitutes life is the presence of a number of such 'organic' compounds, capable of mutually reacting on one another, and thereby giving rise to new compounds which, because of their origin, have no further chemical interaction, and which therefore will form a comparatively stable mantle round the unstable or active groups which gave rise to them."

Thus the cytoplasm is a new environment created between the chemically active groups (chiefly nuclear) and the world at large. It has the functions—(*a*) of elaborating food, (*b*) of protecting the nucleus from deleterious influences, and (*c*) of attracting food to the cell, or of moving the cell towards food. But what, after all, *is* life?

Cycadofilices.

In a paper read before the Royal Society on January 26, Dr. D. H. Scott, F.R.S., gave an account of the structure of a new representative of a group of Palaeozoic plants known as Cycadofilices. The plants included under this head are among the most interesting of all fossil types, as they furnish very important evidence as to the lines of evolution of both ferns and cycads. Dr. Scott has recently had an opportunity of investigating a large number of specimens of a new species of the genus *Medullosa*, discovered by Messrs. Wild and Lomax in the Ganister beds (Lower Coal-Measures) of Lancashire. This first British example of the genus has been named *Medullosa anglica*. It differs in several points from the continental species, and adds some valuable facts to our knowledge of the Medulloseae.

The stem—measuring about 7 cm. in diameter—was thickly clothed with the large leaf-bases of compound fronds, bearing ultimate segments of the form long familiar in the common Palaeozoic genus *Alethopteris;* and branched roots were given off in vertical series between the confluent bases of the leaf-stalks. In habit the plant must have borne a close resemblance to some recent tree-ferns.

The vascular system of the stem consists of three (or locally four) steles, anastomosing and dividing at long intervals. Each stele is made up of a central mass of primary tracheids and conjunctive parenchyma, enclosed by a band of secondary wood and phloem, exhibiting the characteristic structure of recent cycad stems. The stem of *Medullosa anglica* is best described as a polystelic *Heterangium*. The leaf-stalks have the structure of the well-known *Myeloxylon* petioles, which are frequently met with in the calcareous nodules of the Coal-Measures. The course of the leaf-traces is peculiar, and differs from that in any known cycadean plants. The roots, not hitherto known in *Medullosa*, were triarch in structure, and developed a fairly broad zone of secondary wood and phloem.

Without attempting to enumerate the many facts of interest brought out in Dr. Scott's able paper, we may conclude by quoting the author's opinion that while *Medullosa* combines in a striking manner the characters of ferns and cycads, it should not be regarded as having lain "very near the direct line of descent of the latter group."

What constitutes Publication?

THIS is always a vexed question. Those who contend that simple distribution by an author is equivalent to publication should note the remarks of Mrs. A. F. Kenyon in the *Victorian Naturalist* for December 1898, vol. xv. p. 99. This lady printed a List of Victorian Marine Mollusca, and posted a copy to various conchologists, including Professor R. Tate, who criticised its misprints in the journal mentioned. Mrs. Kenyon now says: "As the list was not published at the expense of any society, or offered for sale, I fail to see what right your correspondent had to review, it having been sent to him as an act of courtesy." Why on earth any one should go to the expense of printing matter of this kind "for private circulation only" we never could imagine. All the same we agree with Mrs. Kenyon that her list was not published, and should not have been referred to in public print, whether favourably or unfavourably.

The Discoverer of Kitchen-Middens.

IN a well-reasoned paper, Dr. William Sörensen of Copenhagen asks and answers the question, "Who was the discoverer of the refuse heaps or 'kitchen-middens' of the Stone Age?" The designation which has had so great a vogue is no doubt due to the celebrated and versatile Johannes Japetus Smith Steenstrup, and he has not uncommonly been credited, not only with having invented the telling name, but also with

having first discovered the unexpected nature of the great shell accumulations. He acted as reporter for the trio who, together or separately, at first unofficially, and then as an official committee, investigated those vast leavings of prehistoric luxury. The three scientific men concerned were Forchhammer, Steenstrup, and Worsaae, and the transaction begins half a century ago. But by a careful comparison of dates, and casual notes in a diary, and the precise wording of reports, Dr. Sörensen has clearly shown that the first and second of those authorities were forestalled in the true explanation by the third. While Forchhammer and Steenstrup were still accounting for the position of the seeming oyster-beds, high above the sea-level, on such hypotheses as the depression of the water or elevation of the land since the molluscs had been deposited, Worsaae had the luck and the wit to find evidence that the phenomenon was indisputably due to the agency, not of inanimate nature, but of man. Steenstrup's own earlier researches had contributed to prove the existence of men in Denmark at a time when the shell-heaps were in process of accumulation, so that he readily endorsed a discovery which he may have thought that Worsaae had taken almost from between his teeth. Fifty years ago it was an audacity to believe in men so very ancient as these oyster-eaters. Now we only think of their audacity in eating so many oysters.

The Patagonian Ground-Sloth, Neomylodon.

ZOOLOGISTS are still much exercised concerning the supposed existing ground-sloth of Patagonia, and the Zoological Society of London on February 21 devoted the greater part of an evening to a discussion of its skin. We were the first, last October and November (vol. xiii. pp. 288, 324-326), to recognise the importance of Dr. Ameghino's announcement of the discovery of this remarkable skin fragment, which he briefly described under the name of *Neomylodon listai*. We also made his privately-printed pamphlet generally accessible by inserting an English translation of it in our November number. Two months ago we announced (vol. xiv. p. 171) that Dr. F. P. Moreno, Director of the La Plata Museum, had brought a piece of this identical skin to England and exhibited it to the Zoological Society of London. We now have pleasure in briefly reporting Dr. Moreno's account of the discovery of the specimen, and Mr. Smith Woodward's detailed description of its characters, which formed the basis of the Zoological Society's discussion on February 21, to which we have referred.

It appears that Dr. Moreno found the piece of skin hanging in a tree at a farm near Consuelo Cove, Last Hope Inlet, in Southern Patagonia. On his expressing interest in the specimen, the owner

took him to a cavern where it was said to have been found, buried in earth, two years previously. Dr. Moreno at once began to make excavations, but did not succeed in recovering anything more except some unimportant bones of rodents. He had therefore to remain contented with the skin which he first noticed, and transmitted this direct to the La Plata Museum. He learned that Dr. Otto Nordenskjöld had already taken a piece of the specimen to Sweden, and that some Chilian officers had carried away other pieces; he also had reason to believe subsequently that Dr. Ameghino had obtained a portion. Dr. Moreno's impression was, that the skin belonged to the extinct ground-sloth, *Mylodon*. Although the specimen had an extremely fresh aspect, and had evidently been removed from the carcase by man, he perceived no difficulty in this supposition; for the Mylodonts were known to have been contemporaneous with man in other parts of Argentina farther north, while in another cave in the same district he had already found a well-preserved mummified human body of an extinct race, which was entirely unknown even to the existing Tehuelche Indians of that region.

At the Zoological Society's meeting, Mr. Smith Woodward began his observations by emphasising the remarkably fresh aspect of the skin. A coating of dried serum was even still preserved on the old cut edges. He would, indeed, have unhesitatingly pronounced the skin to belong to a recent animal killed quite lately, had not Dr. Moreno been able to give so circumstantial an account of the discovery. The specimen was doubtless referable to an Edentate, but it was unique even for a member of that order in having the armour of ossicles confined to the lower half of the dermis, while the covering of hair was implanted in every part of the upper half. The ossicles were very similar in structure to the bony plates of the armadillos; in fact, intermediate between the latter and the known ossicles of *Mylodon*. The hair was also like that of the armadillos, and there was no under-fur. Mr. Woodward thought he could recognise part of the left ear and left cheek at one corner of the specimen, which would imply that the skin belonged to the neck-region; and in this case it was quite large enough for *Mylodon*. The bony armour of this part of *Mylodon*, however, was still unknown; the ossicles of the skin of this great beast had only been definitely described from the lumbar region. Hence the impossibility of deciding between the two rival hypotheses of Drs. Moreno and Ameghino. Mr. Woodward said he could only confirm the opinion that the skin truly belonged to a Mylodont ground-sloth; he could not determine the genus without more evidence.

In the interesting discussion which followed these observations, Professor Ray Lankester expressed his hesitation in accepting the problematical skin as that of an armoured ground-sloth. From the structure of the hair and the ossicles he suspected it might possibly represent an unknown group of armadillos; but he anxiously awaited

more evidence. Professor Seeley and Mr. E. T. Newton favoured the theory of Drs. Moreno and Ameghino and Mr. Woodward. Several speakers alluded to the difficulty of believing that the specimen had been naturally preserved in a cavern since prehistoric times; but Dr. Garson thought the discovery of the ancient human mummy in the same neighbourhood was conclusive proof of the favourable nature of the climate.

More about Neomylodon.

THE pieces of the skin of *Neomylodon* collected by Dr. Otto Nordenskjöld have been turned to good account by Dr. Einar Lönnberg, of Upsala, who has just issued a detailed description of them in the report of the Swedish expedition to the south of Patagonia.[1] Dr. Nordenskjöld took his specimen when the piece of skin was about 1·5 metres in length—it is now only half a metre square. He was also first to explore the cave scientifically (in 1896), and there he found the sheath of a claw which might very well belong to a large ground-sloth. Unfortunately, however, there was not a trace of the bones of the animal.

Dr. Lönnberg considers that *Neomylodon* was an animal of the approximate size of a small rhinoceros, or one of the Pampean mylodons; and this leads him to conclude that the living animal stated to have been seen in Patagonia by Lista, and compared to an Indian pangolin, must have been a totally different creature, and not, as Ameghino supposes, identical with *Neomylodon*.

That the latter was a relative of *Mylodon*, and not a cousin of the glyptodonts and armadillos, is considered by Dr. Lönnberg to be practically certain. Not only are the dermal ossicles akin to, although apparently to a certain extent different from, those of *Mylodon*, but the claw is of a mylodont, as distinct from a glyptodont type. With regard to the hair, Dr. Lönnberg considers that in his specimen the outer sheath has in each case been removed, and that only the core remains. And he is thus led to believe that a very close analogy exists between the hair of the Patagonian animal and that of the sloths. If, as he is inclined to think probable, some of the hairs of the former were coated with an alga, there would seem undoubted evidence of bradypodine affinity in this respect. But before the theory that the hairs in their present condition are nothing more than cores be definitely accepted, it would be wise to wait and see what Mr. Smith Woodward has to say on the subject.

Apparently Dr. Lönnberg is of opinion that *Neomylodon* is generic-

[1] "On some Remains of *Neomylodon listai*, Ameghino, brought home by the Swedish Expedition to Tierra del Fuego, 1896," *Svenska Exped. Magellansländ*, ii. pp. 149-170, pls. xii.-xiv. Stockholm, 1899.

ally distinct from *Mylodon*, and also that it has now probably ceased to exist. After mentioning that it might possibly have escaped the notice of the scattered white population of Patagonia, he adds that "it is absolutely impossible to think that this animal, if it was still among living beings, could have eluded the sharp eyes of the native Indians. Even if it had exclusively nocturnal habits, and hid itself during the daytime in the most desolate places, the hunting Indians must have come across it now and then, and they must certainly have observed its tracks, its traces where it had broken off branches and twigs when feeding, its scratchings or diggings in the earth, its excrements, etc."

In all the above there is doubtless much of truth; but, on the other hand, it must be remembered that a very large proportion of the naturalists who saw Dr. Moreno's specimen were inclined to regard it as of very recent origin indeed. And if the creature has been alive a few years ago, there is a strong probability that it still survives. With regard to its generic distinction from the mylodonts of the Pampean, it may be mentioned that many of these lived on to a comparatively late epoch, and also that most, if not all, of the modern genera of armadillos were already in existence during the Pampean period. This being so, it does seem, *primâ facie*, somewhat strange that *Neomylodon* should be entirely of post-Pampean origin, more especially if it is now either extinct or on the verge of becoming so.

Supposed Mesozoic Mammals from Patagonia.

THE preliminary results of Dr. Santiago Roth's discovery of so-called Mesozoic mammals in Patagonia, to which we referred some time ago (vol. xiii. p. 439), are rather disappointing. They appear in the *Revista* of the La Plata Museum (vol. ix. pp. 381-388), and a detailed memoir is promised later in the *Anales* of the same museum. The remains were found by Dr. Roth, associated with bones of Dinosaurian reptiles, in no less than three distinct formations. In one place, moreover, a marine band was intercalated between the mammal-bearing deposits, and this contained casts of shells which are said to occur in the undoubted Cretaceous of Brazil. The mammals themselves, however, are totally different from those primitive types which we have hitherto known to be of Mesozoic age. If they were found in the northern hemisphere, indeed, and if their exact date were unknown, we should not be surprised if mammalogists claimed them to be late Tertiary. Nine new genera are founded, all apparently most closely related to the aberrant hoofed animals which are so characteristic of the Tertiary formations of South America. It seems to us more likely that Dinosaurs survived in the Tertiary fauna of the South American continent, than that highly specialised Ungulata were already

differentiated there in the midst of the Mesozoic fauna; but whatever be the geological result of these discoveries, we await Dr. Roth's completed memoir with the greatest interest.

A Fine Taste in Binding.

In Mr. Thomson's review of Professor Ewart's "Penycuik Experiments" (*Natural Science*, xiv. pp. 203-212) no notice was taken of a feature which should not have escaped a naturalist's eye, we mean the "external characters." It is with these that a biological description should begin, and in this case they are striking enough. We believe that the quaint work called *The Evergreen*, also an Edinburgh product, had its spring number at least clothed in lamb's skin, which was suggestive enough of the gentle bleatings within. It is strange therefore that one of the authors of *The Evergreen* should have failed to remark that "The Penycuik Experiments" are clothed "in zebra." We would congratulate the author and the publishers on this elegant binding in the hope that others will follow their example. How much it would add to the grace of our bookshelves, how much it would lessen the labour of finding a book, if each volume were so to speak indexed by its binding. How ineffably better than any decimal notation if biologists would in their binding rehabilitate the "doctrine of signatures." We read that "Matopo" became strangely excited when he saw a zebra skin; we should like to know what he did when he saw the book.

The True Function of the Thymus.

Dr. John Beard is to be congratulated on having arrived at the solution of a problem which he has had before him for years,—the function of the thymus. Since Kölliker discovered its origin in mammals from the epithelium of a gill-pouch, and stated that the original epithelial cells gave rise to lymph cells or leucocytes, two views have been held regarding this puzzling organ. "On the one hand, Stieda and His have maintained that the leucocytes which always form integral parts of the thymus soon after its first origin have migrated thither from the exterior, possibly from the mesoblast. In this conclusion they have been supported by the researches of Dohrn, Gulland, and Maurer, and by almost every text-book of embryology and comparative anatomy published since 1879. On the other hand, Kölliker has stoutly maintained his original position, and the results of his investigations have been emphatically confirmed by Prenant, Oscar Schultze, and Beard." So far the historical aspect.

But Beard has now shown (*Lancet*, January 21, 1899) that in the

smooth skate (*Raia batis*) leucocytes appear at a time when the spleen has no existence, when there is no rectal gland nor lymphoid structures of any sort. The leucocytes are then present in the epithelial cells of the thymus primordia. Thence they soon begin to migrate, emerging in crowds which cause larger or smaller "breaks" at various places on the contour of the thymus. As the author puts it:—"It is Kölliker's great service to have shown that leucocytes arise in the thymus from its original epithelial cells; to Gulland's researches we owe the result that the first leucocytes are found in the mesoblast in the neighbourhood of the thymus; and, finally, it has fallen to my lot to show that the first leucocytes arise in the thymus from its epithelial cells, and that thus it is the parent source of the leucocytes of the body." We are not clear as to the logic of the "thus" in the last sentence, but the discovery is a triumph, and we eagerly look for a full paper with illustrative figures.

Antherozoids in Gymnosperms.

THE long-expected account of antherozoid formation in *Cycas revoluta* has at length appeared in a form which renders the extremely valuable results obtained by Mr. S. Ikeno available in all their details to the ordinary European reader (*Jahrb. f. wiss. Bot.* Bd. xxxii. 1898). Several notices which have from time to time appeared in various publications have already made us acquainted with the general outline of the research, but we have now before us for the first time a complete account of the process of fertilisation in *Cycas* as it takes place under normal conditions in the natural habitat of the plant.

The development of the archegonium differs but little from that in other Gymnosperms, except as regards the enormous size, 4 mm. × 1 mm., reached by the adult organ. The most remarkable results obtained are, however, those relating to the germination of the pollen grain, a process for the completion of which a period of three months is required.

The greater part of the ripe spore is composed of a large round "embryonal" cell, accompanied by two much smaller "germinal" cells placed one above the other close against the inner side of the wall. The pollen tube arises in the usual way from a thin area in the wall of the embryonal cell, the nucleus of which accompanies the apex of the tube as it bores its way into the nucellar tissue, where it branches and apparently soon becomes quiescent. Meanwhile the two germinal cells have become spherical, at the same time increasing considerably in size, especially the inner one, the nucleus of which divides in a direction at right angles to the axis of the pollen tube, without however any formation of a cell wall. One of the resulting daughter nuclei commences to grow rapidly while the other remains of small size, and is ultimately crushed against the wall by its more successful sister.

A similar occurrence has been recorded by Strasburger (*Histol. Beitr.* Heft iv.) in certain Coniferae, but in this case the division is parallel to the axis of the pollen tube, and a distinct wall is formed. With this difference, however, the processes are identical, and the crushed nucleus of Cycads must be looked upon as homologous with Strasburger's "stalk" cell, while its sister nucleus corresponds to that of his "body" cell.

At this stage the embryonal nucleus begins to move back from the apex of the pollen tube, and eventually comes in contact with the body cell, now considerably enlarged, elliptical in outline, and filled with dense protoplasm. The exine covered remains of the pollen grain thus contain, besides the body cell, the outer germinal cell, the nucleus of the embryonal cell, and that of the stalk cell, all of which presently swell up and disappear, being apparently absorbed by the body cell, which continues to increase in size and eventually divides, the daughter cells remaining for some time connected by a strand of faintly staining cytoplasm. The plano-convex cells which result from this division lie with their flat surfaces in contact, and become the antherozoids. Each consists of a large nucleus enveloped in a comparatively thin sheath of cytoplasm, in which not far from the centre of the convex side there appears a short band, the origin of which is tentatively ascribed by the author to the centrosome. This band, at first composed of granules which soon fuse to form a filament, comes in contact with a beak-like process thrown out by the nucleus, increases rapidly in length, and finally describes four or five complete spiral convolutions directly beneath the convex surface of the antherozoid. Its outer face is clothed with cilia, which, formed at first within the cell, ultimately project beyond it into a spiral groove running parallel to the band, and indenting alike cytoplasm and nucleus. The antherozoids when ripe escape from the pollen tube into the fluid-filled space within the membranous remains of the nucellus; here they swim to the neck of the archegonium and thus enter the egg cell, where the nucleus slips out of its cytoplasmic sheath, leaving it to disappear in the upper part of the cell while it moves down to the centre to fuse with the nucleus of the egg.

The Broad Path.

EVERY one has no doubt heard of the ingenuous youth who defined Grimm's Law as the law whereby any consonant could be replaced by any other, but we have sometimes wondered whether a definition of biology on similar lines might not prove acceptable to some of its students. As thus—Biology is the science which proves that everything is in process of being converted into something else, and that this has an important bearing on human life. We recommend the definition to the notice of the versatile author of "Cycling: its Effect on the Future of

the Human Race" (*Medical Magazine*, 1899, vol. viii. pp. 128-135). We admit that it is defective in that its phraseology is simple, and does not involve those "exact distinctive terms" without which "clearness of understanding is impossible," but our only apology is that the variety of biology to which we have been accustomed does not enable its devotees to attain the clearness gained by describing man as a "degenerating, quadrupedally-ancestored biped." We are afraid also that our biological training did not include the study of the "successive stages in the progress of the human baby towards the attainment of bipedal progression," in spite of the fact that a "knowledge of the outline of such a history is part of an ordinary scientific equipment." We regret these deficiencies the more because we think that "science" like that of Mr. Buckman's article—which, of course, we take as a *jeu d'esprit*—must have many advantages. When one gets habituated to the harmonious multiplication of adjectives, and learns to indulge in bipedal locomotion instead of taking a walk, the broad sweep of the generalisations and the easy superiority to petty detail must have a wonderful fascination, and besides it must save trouble. Biologists of the old-fashioned type have, we believe, spent laborious days in investigating the structure of animal parasites, and yet how needless was their labour! Could they but reach the true light, they would realise the simplicity of nature, would understand that parasites have "reverted to ancestral conditions," where the "body of the organism is nourished by endosmotic action." Similarly, physiologists need no longer toil and experiment, for their problems can be easily solved. Would they learn the cause of the symptoms of alcohol poisoning? Obviously these can be readily explained as a "return towards the quadrupedal stage." Would they as physicians know the cause of baldness in man? Again what is it but reversion? Surely the beauty and simplicity of this method of reasoning must be apparent to all. It is true that it leads to somewhat revolutionary results; thus we learn that the whale is an "instance of return from a higher to a lower state," for it is a "mammal which has gone back to the fish-like form," but we must be prepared to sacrifice a few prejudices to a method which yields such a copious amount of information on so many diverse topics. Does it not teach us that one of the great advantages of cycling is that "the weight is rolled along," while "in bipedal locomotion the body is reared up on end" and "carried, in a most disadvantageous manner, alternately on one limb," and who would not sacrifice much to obtain so priceless a bit of information? Let whosoever will therefore roll with our author down the broad path, regardless of the stubborn few who attempt to bipedally progress up the narrow one. We have entered this protest against science made easy, but surely the sense of humour is not so dead amongst us that it will be supposed that we mean more than that Mr. Buckman has lapsed in his article just as Homer may have nodded.

ORIGINAL COMMUNICATIONS.

The Development of Rivers; and particularly the Genesis of the Severn.

By S. S. Buckman, F.G.S.

I recognise the difficulty of the task. I do not pretend to be able to perform it adequately; but I have the hope of placing the subject on what may be a somewhat surer basis for future work.

I. *Introduction.*—A few words as to the evolution of my ideas, showing how greatly I am indebted to other people, may not be amiss. In some topographical articles (1), and in a communication to the Geological Society (2) in reference to some valleys now holding tributaries of the Severn, I pointed out that their upper parts could only have been excavated by streams which were tributaries of the Thames; that therefore there must have been high ground—lost Cotteswold Hills, in fact—to the north and west of the present escarpment; and that the Thames must have been a larger and longer river than now. I said that the diversion of a west-to-east stream, tributary to the Thames, must have been brought about by the working back of a Severn tributary—the one which now forms the Chelt; and that in course of time the present Chelt must serve the same trick on another Thames tributary, the Coln, because it could give so much quicker fall.

But in extending the Cotteswold Hills farther to the west, I certainly had no idea of filling the Severn valley completely with Jurassic rocks, and supposing the non-existence of the Severn itself. However, when I read the paper by Osborne White (3) citing the opinion of Professor W. M. Davis (4), that, in effect, the Severn valley was filled with Secondary strata, and that the Thames streams originally headed back in Wales, what was till then obscure became suddenly clear: the phenomena of the Severn and its tributaries seemed easily understandable. And the more the idea is worked out the more does the evidence in its favour seem to accumulate.

I ought perhaps to make this somewhat clearer. Of course it has always been recognised that the Severn valley was originally filled up

with Jurassic rocks and buried beneath Cretaceous strata; but it had not been understood that the Thames streams flowed across this area for a long time before the Severn had existence. It seemed to be supposed that the Severn and the Thames were contemporaneous, that the Severn cut a channel between Palaeozoic and Mesozoic rocks, and so initiated the Mesozoic escarpments; that the Cotteswold escarpment extended farther west to nearer the present position of the Severn; and has only been worn back therefrom by the further cutting done by the Severn.

Yet I afterwards learnt that some dozen years before Professor Davis put forth his views, Dr. T. S. Ellis (5), of Gloucester, had imagined the possibility of rivers flowing across a non-existent Severn valley. He says, "If we could have it admitted that the trend of the country was originally to the east before the present [Severn] valley was formed at all, following a slope as the Cotteswolds now dip; then one could imagine a stream flowing through the gap between Malvern and May Hills along the line of the Leadon and escaping over the Cotteswolds at Witcombe, so first marking out one of the west-to-east streams of which apparent signs remain."

To his paper he appends a map showing the application in three cases:—A. The Severn to Tewkesbury flowing up the valley of the Swillgate to Cheltenham, and up the Chelt valley to the Coln at Andoversford; B. The Leadon flowing past Gloucester and over the Cotteswolds near Birdlip, and so into the Thames system; C. A river from west of May Hill flowing up the Stroud valley along the course of the Frome.

In his arguments for his theory from the physical features of the country he makes another good point. "The peculiar course of the tributaries of the Severn in our district seems to have a bearing on my suggestion that the first streams flowed across the line of our valley. It is remarkable that nearly all those on the left bank flow towards the river in a direction against that of the Severn itself. . . . Can these tributaries on the left side be occupying channels originally marked out by streams flowing in the opposite direction?"

Lately I have had the great advantage of accompanying Professor Davis to some of our Cotteswold streams, and he has urged me to give an account of the Severn.

This introduction is rather long; but I wish to show my indebtedness to others. I feel that the ideas to be set forward are not mine in any great degree. And, in fact, I know not how much is from myself, or how much has been suggested by the written or verbal communications of others.

II. *General Considerations.*—At the first initiation of the drainage of an area the direction of streams is determined by the dip. Such streams are termed *consequents* by Professor Davis (4). Now it is necessary to consider what would happen in a tilted area composed of rocks of varied power in resisting denudation, and different porosity.

For the purpose it may be said stronger and weaker rocks, also pervious and impervious.

The stream cuts down its beds so that different rocks outcrop in the stream-bed, and are exposed on its flanks. Weak rocks are denuded faster to make combes, impervious rocks send forth springs. The springs and combes become side-streams and valleys. The side-streams are *subsequents*. Now consequents cannot get any better off than at first: they get all the drainage and cannot get more. But subsequents become better off the more they work back. The consequents are dip-streams, they are best off when they start. The subsequents are strike-streams —they are worst off at starting, and best off at finish.

A strike stream has a dip side and an antidip side. It obtains all the drainage of the dip side by the former streams. If it can give the water a quicker fall to a lower level in a shorter distance,[1] it will start streams on the antidip side. Antidip streams are *obsequents*. Like subsequents they become better off the more they can work back. They are really stronger than subsequents, and can in time obtain practically all the drainage of a given area.

In the case of an anticline a consequent could grow by developing obsequents beyond the axis. The Salisbury Avon seems to be doing this in connection with the Pewsey anticline, taking the water and destroying the south drainage area of the Kennet. Artesian action too is possible —it may obtain water which falls far to the north of the Kennet.

In a drainage area which had not suffered previous denudation subsequent streams would be started along anticlinal axes, because there the impervious rock, and hence the water-level of the upper area would first be reached.

In an area which had suffered previous denudation so that different rocks outcrop, the same would apply where there was a wide expanse of a rock of uniform composition. The head-waters of the Salisbury Avon in the Pewsey valley, and the growth of the original Sevenhampton branch of the Coln in the Charlton Abbots valley, are examples. In part this may explain the preservation of synclinal areas.

But in such a drainage area, where stronger and weaker rocks outcrop not far apart, the subsequent streams will be developed along the strike of the weaker rocks. The Ock, and the Thame, and the Thames above Oxford, are examples. So, in fact, are rivers which drain along the strike of New Red Sandstone, Lias, Oxford Clay, and so on.

But this is a digression. What I want to point out is this, there are started, first, dip-streams, secondly, strike-streams, and thirdly, antidip-streams. Where would their position be? Naturally in the lowest ground—the valleys of the beheaded dip-streams or consequents.

This seems to be an important matter to consider in drainage restoration. If obsequents tend to occupy and work back in valleys of

[1] This is important: it is not necessarily a shorter route to the sea, but an immediate quicker fall.

consequents, there would be established a principle of permanence of river valleys, making drainage restoration a certainty. The valley would remain a valley even though denudation had removed its sides far below the original floor of the consequent stream. There is Dr. Ellis's suggestion with regard to the left or antidip tributaries of the Severn; as it would be put now, they are obsequents which occupy and have deepened valleys of consequents, or Thames streams. Diagrammatically the history has been this:—First, consequents flowing N.W. to S.E. (Fig. 1, A), secondly, consequents cut into by a subsequent, and obsequents started up the consequent valleys (Fig. 1, B). Then the obsequents proceed farther up the consequent valleys, their

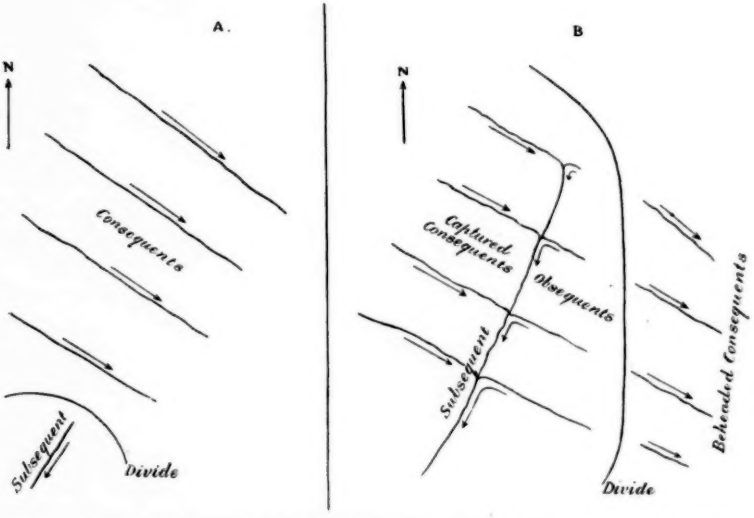

FIG. 1.—Theoretical diagrams illustrating (A) consequent streams threatened by a subsequent, (B) consequent streams captured by subsequent, and initiation of obsequents.

relative lengths according with the successive dates of capture of the consequents. Naturally, then, the obsequents should have a direction S.E. to N.W., and they have. See the sketch map (Fig. 2), which illustrates the actual result, closely in accordance, it will be seen, with the theoretical diagram (Fig. 1, B). This matter is very important; and it follows from considerations of these cases that the beheaded consequent and the growing obsequent should be found occupying opposite sides of the divide with in many cases a continuous valley (Fig. 3).

Therefore in such valleys, which would form passes, the obsequent and consequent streams should be opposite to one another.

From many actual cases I select a rather striking example:—Two cases are shown in Fig. 4, the Isborne and the Coln, in a deep pass: a branch of the Windrush and a branch of the Isborne—the

divide in a slight depression. The Coln had formerly taken off the head-waters of this Windrush branch.

Professor Davis (7) gives an example:—A branch of the Meuse and a branch of the Toul (Moselle) opposite.

This brings me to another matter—passes and depressions often produce the feature of breached escarpments. What is the significance of breached escarpments, and what do they point to in the way of river restoration? To understand them it is necessary to classify them, bearing in mind that mainly they represent beheaded valleys. According to the history of the valleys they have to be classified. There are:—

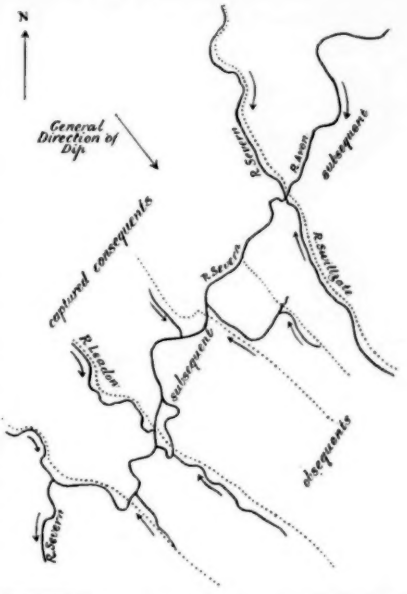

FIG. 2.—Sketch map of the present arrangement of the Severn and its tributaries near Gloucester. The dotted lines indicate the supposed courses of the original consequents. From the Ordnance Survey, Scale, 1″=4 miles.

I. Valleys opening out headward on a plain. A stream drains the plain, and perhaps more beyond; and it flows down the valley with the dip. Examples:—The Goring gorge; the breach in the Chalk

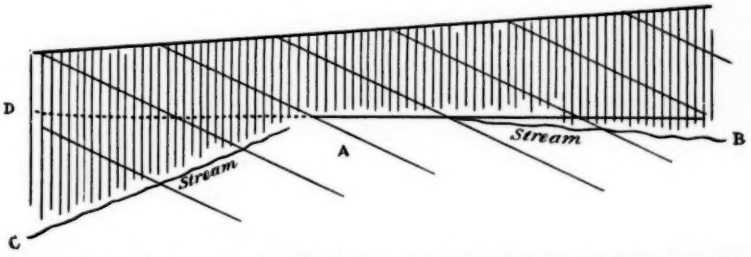

FIG. 3.—Section of valley crossed by divide at A. B is beheaded consequent stream, C is the obsequent stream. D is the original level of the floor of the formerly extended valley of the consequent. This really shows what I have termed a duplicate valley (6). Above D B is one (the original valley); D A C is the second valley. The inclined lines show dip of strata (exaggerated).

escarpment at Upavon, where the Salisbury Avon enters, draining the Vale of Pewsey.

II. The valley is similar, but no stream flows through it. If it holds a stream at all it is only in the lower part. Example:—The

Ogbourne gorge, in the Chalk to the south of Swindon towards Marlborough, along which runs the Roman road from Cirencester to Winchester.

III. The valley contains a divide from which the floor slopes both ways: it holds two streams—one flowing with the dip and one against (Fig. 3). Examples:—The pass from Cheltenham to Andoversford; the pass from Winchcomb to Andoversford; the Moreton valley.

IV. The valley contains an antidip stream only. Example:—The Stroud valley.

V. The valley opens headward high above the plain; it only contains a stream in its lower part, if at all. Examples:—More or less

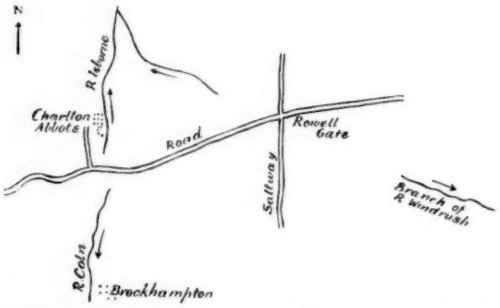

FIG. 4.—Opposition of obsequent and subsequent, and obsequent and consequent, streams near Winchcomb. Scale 1″=1 mile.

dry valleys extending back from the Cotteswold escarpment—a valley leading from above the Vale of Pewsey to the Kennet, in which runs a road from Marlborough to Alton Priors.

To consider the development of these valleys and the escarpment breaches to which they give rise. I. This is due to the cutting down by a consequent stream in the ordinary course of events. II. is due to the same cause; but there is a later phase: the original excavating stream has been tapped and diverted before reaching the gorge. III. This is a further development of II. An obsequent stream has been started, which excavates the original valley-floor of the consequent stream, farther and farther back. This type is found in all stages of development up to IV., in which the original consequent stream has been nearly driven out of its valley, and the valley-floor has been trenched nearly to the mouth of the original valley.

The first type of valley holds a through stream now. The others are considered to have held through streams formerly. Wherefore when they are recognised, such through streams may be given them in river restoration, and according to the relative depths to which the cutting of the original valley-floor has been carried out, so may the further stage of river development be surmised—whether a stream captured its neighbour before it was captured itself, and so forth.

The escarpment breaches connected with these valleys must be separated from those connected with the type of V., which indicate not a through stream but merely local traffic. Their history has been this: when the escarpment stood farther forward there was sufficient collecting ground for the rainfall: a spring could issue at the back of the escarpment and its stream formed a valley; the escarpment has been worn back; the collecting ground has gone; the spring has disappeared; the escarpment has been worn back farther; the head of the valley with the place where the spring once rose has been taken off. The head of the valley forms a notch or breach in the present escarpment.

In drainage restoration a through stream with a distant source must not be accorded to valleys of the type of V., breaching escarpments, only a short local stream must be given.

Among other numerous phenomena of rivers and valleys one further feature must be noticed now, as it is important in drainage restoration. It presents itself as a sharp turn in the course of a stream; not a local twist, but an entire change of the general direction.

When a subsequent or obsequent stream captures a consequent stream an abrupt angle is generally formed at the junction. This angle is called the "elbow of capture" by Professor Davis.

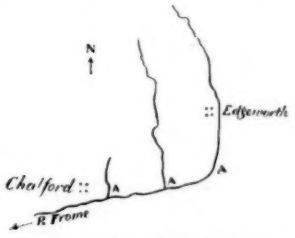

FIG. 5.—Elbows of capture near Stroud.

Fig. 5 shows elbows of successive capture (A, A) of Thames consequents by the obsequent Frome; Fig. 6 illustrates the restoration of the drainage as it was before the captures had been effected. And at the base of the diagram, below Ashton Keynes, is seen the elbow of capture (A) of the there subsequent Thames having taken the Churn. It was the continuation of the Churn which cut the Ogbourne Valley to Marlborough. It was the capture of the Churn by the Thames which has caused that valley to be different from the Goring gorge.

The elbow of capture must be distinguished from a right-angle bend made by the joining of a tributary subsequent to a consequent stream; and especially from the bend which may be at the head of a consequent stream which has been shortened by the successful growth of an obsequent, so that the subsequent stream becomes the longer. The Upper Kennet, where it turns west to the south of Avebury, is a case in point.

It has been necessary to put forward these general considerations, before attention could be devoted to their particular application. Yet again, before they can be applied to the history of the Severn, the development of the Thames must be noticed.

III. *Development of the Thames.*—As seen at the present day the

area of secondary rocks drained by the Thames appears to have been carved out of an original plain which cut across the edges of the strata. It would be a plain of diverse stratal composition.

But when the area was first upraised, there must have been a plain of uniform stratal composition. For short these two plains may be called "a uniform plain" and "an outcrop plain." The outcrop plain has been formed by the removal of the superincumbent strata. But how?

The usual idea is marine denudation. Professor W. M. Davis argues against this (4). He considers the outcrop plain to have been formed from the uniform plain by subaerial denudation—that the

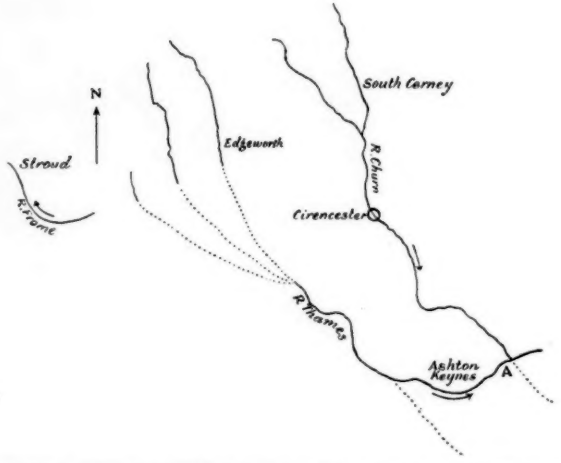

FIG. 6.—Restoration of Thames drainage before capture by the Frome. Elbow of capture of Churn by Upper Thames at A.

rivers cut the country down to a low level: he calls this the first cycle of denudation. The low level plain was subsequently upraised to become a new high level plateau. It is about 700 feet in the Chilterns and about 1000 feet in the Cotteswolds. After the upraising the rivers commenced to cut down afresh—it was the second cycle of denudation.

To me the difficulty seems to be this. Before an area could be worn by river-denudation to a low level of little relief, the third phase of river-development would have reached its possible maximum: a most important system of obsequent streams would have been developed, and they should be as long or even longer than the beheaded consequent streams.

These important obsequent streams are not found. Even at the present day the obsequent streams are not nearly so long as they may be expected to become in order to establish that drainage equilibrium

which must be necessary to wear a given area down nearly to base level.

Drainage equilibrium would be established when the distance from the divide to the sea is the same in either direction. That would be brought about by the growth of obsequent streams.

It seems necessary to suppose that drainage equilibrium would be established before a given area could be worn down to base level.

I confess that it is easiest to start the Thames drainage afresh on an outcrop plain representing a base-level of erosion—a line drawn from about 1300 feet at the Malverns to about 700 on the Chilterns. Starting afresh, however, means marine denudation.

In this plain the Chalk would extend to about Witney, the Severn valley would be filled with Jurassic strata, the Inferior Oolite extending about 3 miles to the west, and the Lias to the Malverns.

An outcrop plain, produced by subaerial denudation, might have been formed in Eocene or Miocene times; but by marine erosion it only seems possible in early Pliocene. That does not seem to give sufficient time. All but the latest details of the Severn valleys are Pre-Pleistocene. However, suppose an outcrop plain, and consider the drainage.

The plain has a general south-east dip, so far as the main area of England and part of Wales is concerned. But this plain seems to have been bounded on the south by a more or less continuous and irregular line of hills—the anticlines of the Mendips, of the vales of Pewsey and Kingsclere, and of the Weald. The consequence was that the drainage of what may be called the great central plain was towards the south-east. But at the southern end of the plain it was checked by the anticlines; from their north side came streams draining northwards, and hence the drainage was given an easterly turn at the south of the plain. Fig. 7 illustrates the position of the supposed lines of original drainage and the positions of the anticlines.

The initiated streams are all consequents; but as they cut down their valleys subsequent streams would be started.

As Professor W. M. Davis points out, the Ock and the Thame are both subsequent streams. So is the Thames from beyond its junction with the Churn to its junction with the Evenlode. This Berkshire Thames was evidently developed before the Ock, by a subsequent growth working west from the Evenlode; it successfully cut into and diverted all the Cotswold streams. Evidently the Windrush once joined the Thames direct somewhere near Abingdon; but the Evenlode, perhaps helped by the Cherwell, had so successfully lowered the country around Oxford that curiously enough it was able to divert the Windrush to a more immediate lower level though giving it a slightly longer course. This was done by the initiating Berkshire Thames subsequent.

The Evenlode was a very large river, draining by one branch the

north Welsh mountains, by the other the west side of the Pennine range.

East of it was a south-easterly extension of the Dove. The Evenlode soon captured this by sending out the Cherwell as a subsequent stream; and on the other side it captured all the Cotteswold streams.

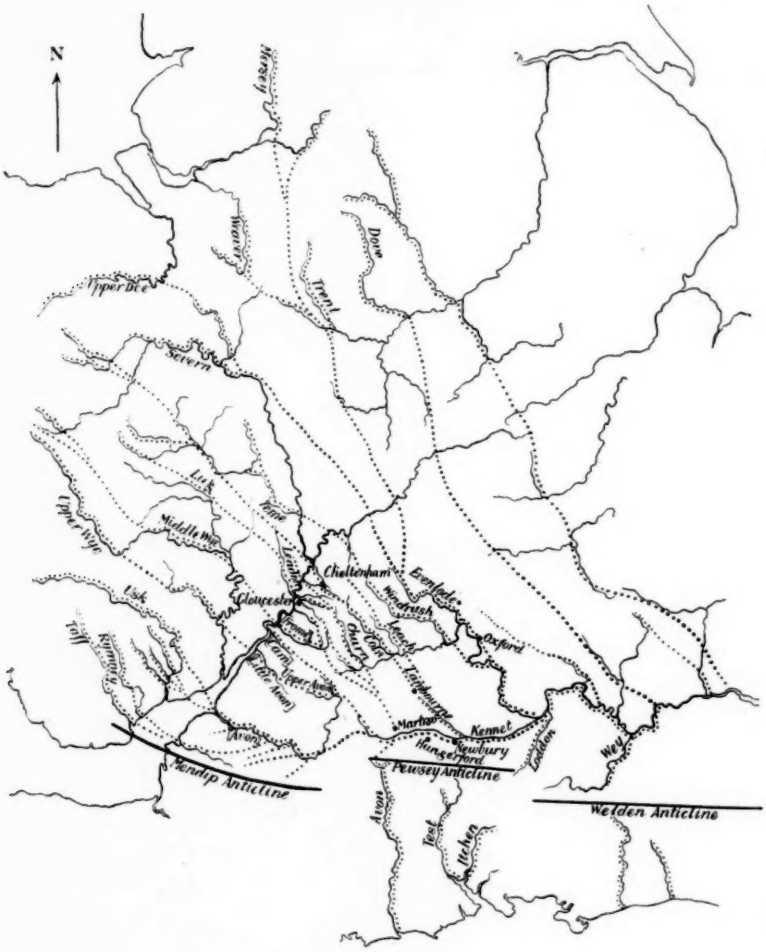

FIG. 7.—Original Consequent Streams.

The Kennet was the other important river. It originally drained Mid and some of South Wales. But the Evenlode robbed it of the Mid Wales water by cutting off the Cotteswold streams. So the Evenlode—including the Thames from Oxford to Reading—was the bigger at starting, and grew bigger in consequence, by robbing the

Kennet. And it is the bigger now; for the Kennet system was the first to be attacked from outside.

But before detailing the incidents of that attack it may be remarked that, on the evidence of the Cotteswold breached escarpment, the Mid Wales or ultra-Severn drainage had been collected into two main channels—the rivers which cut the upper valley at Stroud and at Cheltenham. The latter, the original Coln, seems to have been the most important; by developing subsequent streams it beheaded the Windrush on the north at Winchcomb,[1] and the Churn on the south at Gloucester, thus gaining parts of the Teme, Lug, and Leadon.

The Middle Wye-[Frome][2] does not seem to have made any important captures; but it is surmisable that it gained the Upper Wye, while the Middle Wye and the original Coln lost their head-waters to the Upper Severn extension of the Evenlode. Thus, in what may be called the second phase of the Thames system, the west and north-west drainage had been collected into five principal channels just east of the present Severn—(1) The valley between Dundry and the Mendips; (2) the valley north of Dundry; (3) the Stroud valley; (4) the Cheltenham valley; and (5) the Moreton valley. Of the streams in these valleys the first two joined to make the Kennet, and the other three joined, two by capture, to make the Evenlode-Upper Thames. Then came the attack from outside—the headward growth of a Bristol Channel stream draining to the south-west.

IV. *The Growth of the Severn.*—Not to go too far back, it is probable that the Jurassic rocks which stretched from north-east Somerset to Glamorganshire had much to do in determining the position of the Severn. Possibly a western extension of the Carboniferous Limestone of the Mendip axis—represented by the Holmes—strengthening the Jurassic rocks, prevented or rather delayed headward growth of what may have been a small river with a strongly tidal estuary; and this might have caused the water to be turned south-eastwards to destroy the Jurassic rocks of Sedgemoor and Bridgewater.[3]

But the barrier was broken down, and the Taff and Rhymney were captured. Next the Usk system was obtained.

The largest and most important obsequent stream ought to be found on the south side of Dundry; but it is not—the Avon on the north side is so. That is to say, the longest obsequent has been developed in the valley of the later beheaded consequent, where there has been the

[1] This subsequent stream was developed along an anticline.

[2] The river-name in brackets indicates the predecessor which occupied the valley and flowed in the other direction.

[3] Probably the Dorset Stour represents the direction of the drainage just south of the Mendips, and it originally headed in Glamorganshire, crossing the non-existent Bristol Channel. Then the Parret, the Yeo, etc., are the obsequent streams developed when the Channel, or perhaps it was then only a south-west flowing stream, headed back towards Burnham. It will be seen that the obsequents have had time to become as long as the consequent Stour.

less time for the work. The probable explanation is that the date of capture was not very different, and that the Usk-[Avon] valley was originally the deeper, having received the larger drainage; and so it gave the obsequent stream a better chance.

The Bristol Avon has become the longest obsequent branch of the Severn north of the Mendips. In working back it takes off the waters of the Kennet, and prevents much westward growth of the west subsequents of the Salisbury Avon.

The long eastward course of several Avon tributaries, especially on the south side, points to former eastward flow of the Avon's predecessor. The tributaries join the present Avon in more or less opposition to its stream.

After the Monnow lost its head to the Middle Wye it is possible that it was itself captured by a subsequent branch developed northwards from its then stronger companion the Usk-[Avon]. And when the Usk group was captured by the Severn, this subsequent, which could give a much quicker fall, continued to grow northwards to Hereford, and captured the Wye group, thus beheading the Stroud valley stream. This subsequent would have been developed in Mesozoic rocks from about Chepstow, somewhat along the line of the present Lower Wye; but, probably, the difficulty of cutting through Carboniferous Limestone prevented the Severn working back in that direction, and it found it easier to break through the Palaeozoic barrier at Tortworth,[1] whereby it gained the Forest of Dean drainage.

The remarkable horse-shoe curves of the Wye can scarcely be accounted for by this history. They had perhaps been previously developed in an earlier river flowing over a low-lying area of Carboniferous Limestone during Jurassic times. The valley of this river was subsequently buried beneath accumulating Mesozoic deposits. After the capture of the later stream this infilling was easily re-excavated.

It was pointed out to me by the late Edward Wilson that the Clifton gorge of the Bristol Avon was first excavated in Triassic times, because it contains Dolomitic Conglomerate; therefore the lower part of the Bristol Avon occupies an old pre-Jurassic channel. A similar history may attach to the present Wye.

As the Severn captured the Forest of Dean drainage it started obsequent streams towards Dursley and Stroud. The Stroud obsequent —the Frome—has gradually abstracted the head-waters of the true Thames. Very noticeable is the elbow of capture, where the eastward working-back Frome has broken into small south-eastward flowing Thames consequents, and directed them to itself.

According to the theory the Frome should not be so important an obsequent as the Bristol Avon, because it has not had so much time to work back since its predecessor was captured,—and it agrees.

[1] That it thereby followed the strike of weak strata may have been an important factor.

When the Middle Wye was turned into the Severn system it still continued the northward subsequent, which of course may have been initiated as a tributary when the Middle Wye belonged to the Thames system. That subsequent captured some of the Lug, which, however, seems to have already suffered from the original Coln (Upper Teme). The state of affairs at this date is shown in the sketch map, Fig. 8.

Meanwhile the Severn was working its way north-eastward, and it successively captured the different portions of the Coln group. It must be supposed that before this took place the Teme had developed a

FIG. 8.—Sketch map. Courses of the streams just before the victory of the Severn in the Vale of Gloucester. Brought about—1, by adjustments of Thames streams among themselves; 2, by adjustments of the South Welsh streams among themselves; and 3, such adjustments in the latter case induced by the headward growth of the Severn. Scale about 20 miles to 1″.

northward subsequent towards Stourport, and beyond, so that when the Coln—into which the Teme then flowed—was captured, this subsequent rapidly developed, and obtained as much of the Upper Severn as had not suffered from the depredations of the Dee. If this had not been the case the Severn would have taken a more northerly or north-easterly direction.[1]

[1] North-eastward, somewhat in the direction of the Warwickshire Avon, should have been the main and largest stream of the Severn, working along the strike of weak rocks. Now it has what may be called a somewhat lopsided appearance—an abrupt change of direction at Tewkesbury. So, it will be noted, it had at a prior stage (Fig. 8). That the Avon did not develop faster is due to the proximity of sea on east England favouring successful growth of the Trent, etc., otherwise the Avon had worked back right into Yorkshire.

Having captured the Coln, the Severn started an important obsequent stream up the valley.

Comparing the obsequent Chelt with the obsequent Frome, it will be seen that the former has done less than the latter, which agrees with the date of capture. The Frome has worked back and captured various consequent streams on the Cotteswold upland; but the Chelt has not yet done anything in this way. It is in a less advanced stage of obsequent development than the Frome. It should have captured the present head-waters of the Coln, but it has not yet done so. It will do so in time, but by then perhaps the Frome will have got the Churn at Cirencester.

I had the advantage of visiting the Upper Coln with Professor Davis. He pointed out to me some most instructive phases in its valley development, corresponding with decrease in its volume. There are three valleys. In the upper valley there are meanders of large

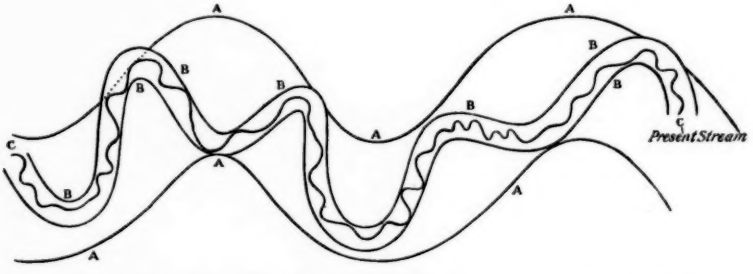

FIG. 9.—Diagram of the phenomena in the Coln valley south of Andoversford. The curves are only very approximate. A A A, curves of upper valley; B B B, curves of lower valley; C stream wriggling in its present valley.

curve, such as a river of large volume would make. In the lower valley the diminished river has been unable to follow the large curves, and it made two turns in each original meander. In the lowest or present valley the river is taking a wriggling course inside the smaller curves. I append a diagram (Fig. 9) to show what has happened; but it requires a map with contour lines at every 25 feet to illustrate clearly such details of river-valley development.

Diminished river-volume is due to two causes—diminished rainfall and diminished drainage area. Allowing that the former has taken place, yet it will perhaps hardly account for all the phenomena.[1] Diminished drainage area is what coincides with the theory set forth. Thus the big curves were cut by the large river, which with its west branch drained the country west of Cheltenham and Gloucester, and by its northern branch—in the Charlton Abbots valley—drained the country north of Winchcomb. The smaller curves were cut by the

[1] The so-called pluvial period of post-glacial times will not fit here, except to the details of the lowest valley; for the date of the upper valley must be Pliocene.

river when it had lost all its western branch, and only received the drainage down its northern valley. In turn its northern branch was beheaded, and its drainage-area further and further diminished by the growth of the obsequent Isborne. So the river is reduced to its present small volume—a little wriggling brook.

Having stood on the Chiltern Hills overlooking the Goring gorge and noted the details of that scene, it is not difficult, when subsequently viewing the Cotteswold escarpment and the old West Coln breach from the south end of the Cleeve Hill plateau, to transfer the details of the former scene to the latter, and imagine the aspect of the Cotteswold uplands and river-system when its stage of development was the same as now belongs to the Chalk escarpment. That is the picture of what the original Coln river-system—the river which cut the big meanders—must have looked like before its capture by the Severn. Between Cleeve and Leckhampton Hills there was a broad river-valley perhaps some 250 feet deep. In that valley a river quite as big as the present Thames at Goring; and stretching away to the west, right to the Malverns, there was a broad plain of Lias about 250 feet below the Cotteswold escarpment. The Upper Lias corresponded to the Greensand series, and the Lower Lias to the Oxford Clay of the Oxford district. And across this plain ran the original Coln, much in the same way as the Thames now crosses the Oxford Clay before entering the Chalk Hills; also in this plain there had been developed subsequent streams corresponding in all respects with the Ock and Thame. The stage of river-development now to be seen in the neighbourhood of the Goring gorge was once the stage of river-development appertaining to the gorge which now lies west of Cheltenham. Here this stage of development passed into a new phase by the successful working back of the river Severn.

After what may be called this Severn victory in the Vale of Gloucester, the further development of the river-system may be shortly told. It started or strengthened two subsequent branches—one northwards which captured the Shropshire and Welsh drainage, the other northeastward—the present Warwick Avon[1] which cut off the head-waters of the Thames tributaries from the north; and has developed obsequent branches like the Isborne and the Stour to further shorten the Thames system. While the Thames was thus suffering from the successful growth of the Severn, it had also experienced considerable loss from other rivers. In the north-west its head-waters had been taken by the Dee, Weaver, Mersey, etc. In the north it had suffered from the Trent, as well as from the three parallel subsequents, the Welland, Nen, and Bedford Ouse. From the latter it had suffered very considerably, especially in regard to tributaries which enter near London.

[1] From the lie of the ground south of Tewkesbury it seems reasonable to conclude that the Avon formerly joined the Severn a few miles south of Tewkesbury, and that the present junction at Tewkesbury is a comparatively recent development.

As time goes on the Thames system will suffer most in the west, by the growth of Severn obsequents like the Bristol Avon, Frome, and Chelt, which can give the Cotteswold drainage quicker fall. It may suffer in the south by the growth of the Salisbury Avon.

The Severn system will suffer in the west by the growth of west Welsh streams; and the Severn itself may suffer by the shifting of its drainage westward—thus probably the Usk, further developing subsequent streams working in the Old Red Sandstone, will capture the Monnow and the Wye, giving the latter an easier channel than through the gorge of Carboniferous Limestone.

Conclusion.—To prevent misconception, it may perhaps be desirable to say that "a stream" may be of water or of ice. The latter will follow the drainage lines marked out by the former; it will not make unaccountable changes of direction. Its chief feature is perhaps to overflow its valley; and this might greatly aid the headward growth of an attacking rival.

Many of the statements in this paper may be termed mere speculation, with very few arguments. It is fully admitted. To pursue the matter further, I want much better maps than are now to my hand. I require—(1) A clear river-map; (2) a similar river-map with contour lines, and, in figures, the height of all hills and passes; (3) a similar river-map showing the solid geology, and particularly the dip of the strata; (4) a similar river-map showing the drift geology. There is of course much to be done in a study of the drift in this connection. That has been shown by J. Prestwich (8), J. W. Gregory (9), and Osborne White (3), not to mention other authorities. Therefrom can be obtained valuable aid in regard to the chronology of river-valleys and the direction of old streams.

Thus in the Moreton valley, just on the present divide—at the head-waters of the Evenlode—there is a mass of drift. It shows the Evenlode to have been a big river, that it drained Triassic country, that its waters froze in the north. The ice floated down, bringing Triassic débris, and it melted in passing along the Moreton valley, dropping the débris endwise into a muddy river-bottom. Much of this drift perhaps extended back over Warwickshire, up the original river; and when the Avon broke into this country, taking off so much of the original river as the depredations of other streams had left—which was little enough—it perhaps attacked this gravel, and has been engaged in transporting it westwards, taking it to Evesham, and perhaps into the Severn valley. And the Stour may have been engaged in retransporting northwards what the original Evenlode had taken the trouble to bring southwards.[1]

The Lower Severn has cut down its valley very much since what

[1] This means that the excavation of the Avon valley, below about a 500 feet contour line, has been accomplished since the deposition of the Moreton drift. What then is the date of that drift? (later Pliocene, when the climate was getting cold).

is called "Northern Drift" was deposited. The Upper (Shropshire) Severn has done very much more. But perhaps there are two deposits confused in the term "Northern Drift"—a high-level drift, and a retransported low-level drift.

The question why Triassic débris is found on the Moreton divide, and not on the Chelt or any of the western divides, may be easily answered. Rivers flowing from the west of May Hill, Malvern, etc., country would come—at the level of the Chelt divide—off Palaeozoic rocks straight on to Lias.

But other queries in this connection will occur, to which answers will be necessary.

The date of an obsequent river like the Frome in the Stroud valley can be approximately fixed by the gravels. In the valley just below Stroud there are gravels with mammoth remains. And so the obsequent valley had been deeply, and it would seem widely, excavated before that date of Pleistocene geology. Since that date the river has lowered its bed some 40 or 50 feet.

In the Chelt valley, near my house, are several gravel pits. Up the valley there is almost entirely Jurassic, mostly Oolitic material. Coming down the valley there is found a greater and greater admixture of a quartz sand. Beyond the valley there is wholly sand. Farther west there are Triassic materials.

The sand mixed with the Jurassic gravel is not local—not from the Cotteswold Hills; it is probably from the Trias rocks. How it was brought up the Chelt valley may admit of more than one explanation.

There is much to be done with the gravels. After the Severn had, as it is supposed, captured most of the western drainage, and had started the Avon into the Midlands to see what it could filch, there then remains a long period of history attached to the widening and deepening of the Severn valley.

REFERENCES

1. "Cheltenham as a Holiday Resort," Cheltenham, 1895.
2. "The Cleeve Hill Plateau," *Quart. Journ. Geol. Soc.* 1897, vol. liii. p. 607.
3. H. J. Osborne White, "On the Origin of the High Level Gravel," etc., *Proc. Geol. Soc.* 1897, vol. xv. pt. 4.
4. W. M. Davis, "The Development of Certain English Rivers," *Geogr. Journ.* 1895, vol. v.
5. T. S. Ellis, "On some Features in the Formation of the Severn Valley as seen near Gloucester," *Gloucester Phil. Soc.* 1882.
6. "Observations of a Cycle Tour," *Proc. Cotteswold Club*, 1898, vol. xii. pt. 3, p. 217.
7. W. M. Davis, "La Seine, La Meuse, et La Moselle," *Ann. Géographie*, 1895, No. 19.
8. J. Prestwich, "On the Relation of the Westleton Beds," etc., *Quart. Journ. Geol. Soc.*, 1890, vol. xlvi.
9. J. W. Gregory, "The Evolution of the Thames," *Nat. Sci.* London, 1894, vol. v. p. 97.

Charlton Kings,
Cheltenham.

The True Interpretation of Lamarck's Theories: A Plea for their Reconsideration.

By R. F. Licorish, M.D.

That Lamarck has been misinterpreted and misunderstood from his day to this, appears to me to be due to two causes chiefly, first, the lack of true biological and physiological knowledge in his day, which led not only to the rejection of his theories by his contemporaries, but also gave origin to the second cause of misinterpretation, viz. the obscurity and ambiguity of his language.

Having succeeded, as I believe, in developing his true meaning, I now desire to bring my views to the notice of evolutionists, believing that this interpretation will make clear many obscure organic phenomena, as well as enable us to do full justice to that great philosopher.

In summing up the opinions of biologists on Lamarck Mr. G. Sandeman thus writes ("Problems of Biology," p. 153): "It is certain that this biologist has been, on the whole, misinterpreted, but, on the other hand, it is not easy to be sure that one understands him. And the confusion has taken place chiefly over that key-word of his system —*besoin*, or need." Again he states: "It—*besoin*—probably appeared to Lamarck to have a quite definite meaning requiring no further analysis."

Mr. Sandeman is quite right in the above statement, for, as I shall show, the whole misinterpretation of Lamarck is due to the non-perception by biologists of the true meaning of this need or desire. Moreover, it will be pointed out that Lamarck himself failed to see the true interpretation in some respects, and hence the full significance of his important conclusions and laws.

Mr. Clodd has well said ("Story of Creation," p. 93): "The functions of living things are threefold—nutrition, reproduction, and relation, in other words to feed, to multiply, to respond to the outer world." There is, however, one special relation which, although included in the last of those, yet requires more attention than Mr. Clodd has bestowed on it, viz. the relations which living things have with other living things, especially as regards protection.

At this stage, it is well to consider carefully the nature of functions, because we cannot understand Lamarck unless we keep in

view the functional life of living things, and endeavour to understand clearly the true nature of a function. A function is the use which an organ serves. All living things possess organs, the uses of which are called functions. Moreover, there are functions which require, not only their proper organs, but also other auxiliary organs for their effective performance. But functions, and hence organs, vary in importance. There are certain functions so highly important to life and the propagation of the species, that special centres in the central nervous system are found to preside over and control them, giving origin to the need or desire for satisfying them. Such functions are—nutrition, reproduction, and, to some extent, protection, and it is in response to the need or desire of the organism that such functions are satisfied.

It is doubtful if any one will be found to dispute the importance of those functions, or their connection with the need or desire emanating from the brain for their satisfaction.

Now it is held as a truism by biologists that "the function makes the organ." That this is so must withstand any attempt at refutation, for it is inconceivable that the organ was first to appear, to be subsequently followed by the function. Stress is laid on this point, because the whole interpretation of Lamarck hangs on its recognition. Further, we can well understand that the nature of the function, *i.e.* the manner in which the need or desire presiding over the function is satisfied, determines the structural nature of the organ. Change the method of satisfying the function and we may change the structural nature of the organ. But we must also remember that the most important functions have not only their proper organs, but also other auxiliary organs which are brought into new uses when the methods of satisfying the functions are changed. If, *e.g.*, an animal subsisting on surface vegetation, etc., be driven to climb trees in order to satisfy its desire for food, as it is said the mongoose in Jamaica now sometimes does, while the true organs of nutrition will doubtless undergo little if any change, yet, if the habit through any cause, such as continuous drought, etc., be fully established, the auxiliary organs—the feet and claws—will gradually be modified until they serve fully the function of nutrition, *i.e.* until the feet and claws fully serve the purpose of procuring food. We should then get undoubtedly, in the case of the mongoose, a change towards a squirrel-like type. We thus learn how correlated are different and distant organs in maintaining the integrity of the most important functions.

Bearing the above facts in mind, we are now prepared to consider the theories of Lamarck. These theories he condensed into certain formulæ and laws, which are as follows:—

(1) "That every change which is at all considerable and continuously maintained in the circumstances of each race of animals, effects in it a real change in their needs."

(2) "That every change in the needs of animals necessitates other actions on their part for the satisfaction of the new needs, and, in consequence, other habits."

(3) "That since every new need requires new actions to satisfy it, it demands of the animal which experiences it either the more frequent use of such a part as was formerly less used, so that it becomes considerably developed and enlarged; or the use of new parts which insensibly arise in the organism from the needs, *by the efforts of its inner feelings*, as I shall presently show from known facts. And to arrive at the true cause of so many different forms, and so many various habits, as are seen in the animal world, one must recognise that the infinitely diversified but slowly changing circumstances in which the animals in each race have successively been placed, have brought about in each race new needs and consequently changes in their habits. As soon as one has recognised this incontestable truth it will be easy to perceive how the new needs can have been satisfied, and the new habits taken on, if one attends to these two laws of nature, which have always been corroborated by observation."

"*First Law.*—In every animal which has not passed the limits of its development, the more frequent and sustained use of any organ gradually strengthens that organ, develops it, increases its size, and gives it a strength proportional to the use in question; while the constant disuse of such an organ insensibly weakens and deteriorates it, progressively diminishes its faculties, and finally results in its disappearance."

"*Second Law.*—All that nature has caused to be acquired by, or lost to, individuals through the influence of the circumstances to which their race has long been exposed, and therefore through the predominant use of an organ, or through the constant disuse of a part, she preserves by reproduction for the new individuals which come from them; provided that the acquired changes are common to the two sexes, or to those which have produced the individuals."

Before entering on the further explanation of Lamarck's key-word, *need or desire*, I have to point out an almost fatal error in Lamarck's language when stating the above conclusions. Lamarck therein speaks of *new needs*, but careful consideration of his conclusions has enabled me to see that he really did not mean *new needs*, but that he referred under such a term to new methods of satisfying the customary needs of animals, as *e.g.* the need of food, etc. Instead, then, of speaking of *new needs* he should have said *new uses* of parts in order to satisfy the functional needs of the animal. This error has been, I believe, a stumbling-block to many in interpreting Lamarck. I myself did not see the error until long after I understood the true meaning of his need or desire. Corrected in the light of this discovery his conclusions read thus:—

(1) That every change which is at all considerable, and continu-

ously maintained in the circumstances of each race of animals, may effect in it a real change in the methods of satisfying their needs, and hence new uses of parts hitherto used in a different way.

(2) That every such change in the methods of satisfying their needs necessitates other actions on their part, and in consequence new habits involving new uses of parts.

(3) That since every new method of satisfying their needs requires new actions, it demands of the animal which experiences it either the more frequent use of such a part as was formerly less used, so that it becomes considerably developed and enlarged, or the use of new parts, which gradually arise in response to the desire or need. And so to arrive at the true cause of so many different forms, and so many various habits, as are given in the animal world, one must recognise that the infinitely diversified and slowly changing circumstances in which the animals of each race have successively been placed have brought about, in each race, new methods of satisfying their functions, and consequently new habits, including the new uses of all correlated parts affected thereby, etc.

As regards the two laws of Lamarck, they are correctly stated, and their truth can be perceived by paying attention to the above corrections.

A careful study of the above conclusions as corrected, and comparison with Lamarck's own statement, must put their signification in a somewhat different light from that suggested by his language.

From what has been already stated, it must be understood that what Lamarck meant by *need or desire* was not, as is usually understood, an *indefinite* need or desire, but those needs or desires of animals which lead to the satisfying of their most important functions to ensure their existence and propagation. I myself did not at first understand Lamarck, but on reading of the reversion of the tame pig to the wild boar in the New World, the true meaning suddenly flashed on me in this way: One of the most significant changes in the wild boar from the tame pig is as regards the ear, which in the former is small and erect, and in the latter large and pendent. On reading this statement, my mind instantly saw the analogy between the ear so modified in freedom, and the semi-erect ears of half-tame pigs in the West Indies. I had frequently seen on such pigs the effect of a noise: they would at once erect their ears and scamper off; and I soon saw that the tame pig, escaping from the Spanish settlements in America, would, in order to avoid danger, have more and more frequently to erect their ears to catch the earliest signs of danger, and that this frequent erection, in order to protect themselves, would gradually cause the ears to lose the superfluous fat and develop muscle, with a corresponding decrease in size, until the ear became like that of the wild boar, and thus efficient for the purpose it served in the catching of the earliest sign or sound of danger; and as the modification of character was an innate one—that

is, in response to the need or desire of the animal—the ear, through heredity, would gradually revert to that of the wild boar. And in a similar way the tame pig would gradually lose all its character due to domesticity, and assume those compatible with a wild life. And we see the same gradual modification of ear in dogs that hunt by scent (*e.g.* the blood-hound), the ears, relatively little used, gradually enlarge, and become more and more pendent.

It may be thought that the foregoing has reference only to "use and disuse," and the modification of characters induced thereby; but to take such a limited view of the changes brought about, in accordance with Lamarck's laws, would be wrong. The laws of Lamarck have reference to changes of a far wider and deeper nature than is understood by the use of the phrase "use and disuse." The modifications of characters produced by changes in the circumstances of animals bear, not only on the use and disuse of special parts, but also on all other auxiliary organs which co-operate in satisfying the wants or needs of animals, which changes in their circumstances have affected. When we consider the many different parts or characters which are brought into co-operative action when an animal, through a change of circumstances, is forced, through its desire for food, to adopt a new method of feeding—when we consider, also, the changes in the nutritive organs, which a radical change in the nature of the food may entail—we begin to realise the many changes, external and internal, which changes in the circumstances of animals may bring about.

Lamarck's theory, as here developed, explains how, granted that a change of habits in any species may produce changes in special as well as correlated organs, and may affect, not individuals only, but a species as a whole, new species appear in a comparatively short time, owing to the fact that all such modifications of characters are fixed through heredity, thus accounting for the absence of transition forms in many instances—a fact which research proves.

It may be well to state how, by the Lamarckian theory, the two kinds of characters, innate and acquired, are explained. Innate characters, on this theory, are those derived through the wants or needs of the animal, *i.e.* through the central nervous system. As they are brought about *en masse* they affect both sexes alike; hence, through heredity, they become fixed, and the species is modified thereby. Acquired characters, on the other hand, are derived through the surrounding conditions, such as climate, etc. Their relation with the nervous system is only through reflex nerves. When the whole of the reflex nerves are affected, as in the case of a tropical sun on the skin, in process of time the central nervous system becomes modified, and the acquired character becomes fixed or innate. On the contrary, characters such as mutilations and scars, which only affect a few of the reflex nerves, do not become innate, or capable of being transmitted, because the unaffected reflex nerves, being in the vast majority, maintain the integrity of the

reflex nerve centre—of even that portion pertaining to the affected nerves.

The gradual change taking place in the ear of the tame pig, in response to its desire to escape its enemies in its reversion to the wild boar, is a good example of an innate character; the gradual tanning of the skin, or darkening of the complexion of the Boers of South Africa, a good example of the conversion of an acquired character, affecting the whole of the same reflex nerves, into an innate one; the non-transmission of scars, etc., a good illustration of an acquired character, which, only affecting a few reflex nerves, does not become in time innate, and so capable of being transmitted, except, perhaps, in very rare cases. Innate characters are voluntarily derived; acquired characters, involuntarily.

Barbados, West Indies.

The Case of Doctor Otto Kuntze.[1]

"Y a-t-il une charlatanerie plus grande que de mettre les mots à la place des choses, et de vouloir que les autres croient ce que vous ne croyez pas vous-même?"—VOLTAIRE.

ONE recalls the noisy campaign, undertaken by Doctor O. Kuntze in 1891, and continued in the following years with a view to regenerate botanical nomenclature. By means of a plausible interpretation of the Paris Code, and by grossly altering several of its fundamental articles, M. Kuntze succeeded in giving to his schism a varnish of legality. In spite of his eccentricities one discussed him with more or less deference. Slashing and pugnacious, blowing his own trumpet, obstinate in his fallacies, he rode, not without bluster, his hobby of *priority at any cost*. Putting words in the place of facts, as Voltaire says, Kuntze substituted for *definitions*, prescribed by the Laws of 1867, his trick of *identification* (*Recognoscirung*), and thus resuscitated an infinity of generic names, still-born according to Article 46 of the Code, applying to them his new scale of *demi-nudity* and that of the *fractional priority of* 50 *per cent*, absurdities combated by Alphonse de Candolle up to his last breath.

This resulted in letting loose a mass of about 30,000 species, and about the same number of *nobis*. These *nobis*, according to their author, mattered little to him, so little that several times over he has declared he could only condescend to make concessions on some points of secondary importance, on the condition of acceptance in their entirety of all his "reforms," that is to say all his *nobis*. Besides, his real aim was more elevated. To establish an immutable nomenclature *ad aeternum*, to cut short all disputes on detail, to render every author his due (especially those who have left simple labels)—such a result could only be obtained by iron rules, much more particularised than those of 1867, and provident of endless litigious cases.

Unhappily for Dr. Kuntze, his neo-code did not appear, to the bulk of botanists, based upon "des motifs assez clairs et assez forts pour que chacun pût les comprendre et les accepter" (§ 2, *Laws*). Many of the premisses seemed arbitrary, many of the consequences doubtful, and it was considered annoying that Kuntze wished to impose them on the world, contrary to the 2nd and fundamental article of the Laws

[1] By E. Levier. 8vo, *Florence*, 1898 (Nov.): especially translated for this journal.

voted upon at Paris. It was considered that the frightful disorder brought about by the neo-code would be a hundred times worse than the evil which it pretended to remedy. In fact, from the uncertainty on the subject of a relatively limited number of names, diminishing year by year, thanks to the work of systematists, one falls into complete disorder, into endless ambiguities, into quarrels over useless names, and into envenomed polemics, all which things Article 3 of the *Principes dirigeants* of the Laws counsel one to avoid as one avoids the plague. It was noted, with satisfaction, that Alphonse de Candolle, whose competence in nomenclature is at least worth that of Otto Kuntze, disapproved of many of the theses of the innovator and declared them inacceptable "avec des raisons claires et assez fortes pour que chacun pût les comprendre et les accepter." It was a case of much ado about nothing. The great clamour raised around the *Revisio generum plantarum* gradually subsided; systematists recovering from their first stupor, once more resumed their labours, gleaning here and there in the Kuntzean repertory a few names recognised as valid, and passing the rest over to the archives.

Otto Kuntze did not found a school. No marked and competent botanist, not a group of monographers or systematists, either in the Old World or in the New, adopted fully, freely, or without reserve his rules and his innovations. The Rochester Code, which is the sole direct and collective emanation of Kuntzeism, adopted only the most unfortunate of his superfoetations on the Laws of 1867, *nomina seminuda*, rejected everywhere else, and equally rejected by zoologists. The scientific men who subscribed to the nine articles of the Rochester Code (and they are nearly all the botanists of North America) rallied, by way of revenge, round the starting-point of 1753, scouted by Kuntze under the sobriquet of "*initium ignorantium*," and on several other points set themselves in open opposition to their ex-master, who spared them neither scratches nor epigrams. We learn to-day, from Kuntze himself, that Article IV. of the Rochester Code (*Homonyms*) and the principle of "*Priority in place*" will compel their authors to change the names of 23,300 species. Kuntze, for his part, needing not only more than 30,000, but more than *40,500* changes, in great part different from those of the Rochesterians, the language of flowers will offer this pretty spectacle: the names of the Americans will be unintelligible to Otto Kuntze, and *vice versa*, whilst those botanists who remain faithful to the Paris Code will have to search out the meaning of about 60,000 schismatic and hieroglyphical species. Hash, chaos, Babeldom—that, in seven years, must evidently be the result of the shock given to systematic principles by the *Revisio generum plantarum*. The indifference into which a pugnacious book falls being the worst of deaths, Kuntze judged the moment had arrived to beat his tom-tom again. An advertising pamphlet, with flattering notices on the last page, attesting the excellence of his wares, was distributed *urbi et orbi*,

to announce the publication of a fourth volume of the *Revisio*. With an alluring title-page for readers eager for scandal, this puff is seasoned with some of the tastiest specimens of the last nomenclatural creation of Kuntze, aiming no longer at plants, but at botanists. The new nomenclature is codified, very simply as we shall see. There are only two Articles :—

§ 1. All those who accept the botanical names of Doctor O. K., or who accept 70 per cent of them, are honest men.

§ 2. All those who are against the botanical names of Doctor O. K. are "ignoramuses," "pettifoggers," "confusionists," "liars" (Lügner), "infamous forgers," "downright idiots" ("senile" and soft-headed), "Jesuits," "sheepsheads," that is to say "imbeciles," "idiots"; better than that, "jugglers," "harlequins," "swindlers," "pickpockets," "dangerous quacks," "bullies," and above all "anarchists."

All these qualifications between commas are culled *passim* from the four volumes of the *Revisio*. There are some more spicy still, but I pass on.

What a relief to the camp of the "executed," and what a riddance! A man of science who gives his reasons and defends them, even with vivacity, one takes seriously. One admits, occasionally, a stronger word, provided it does not smack of the gutter. One inoculates oneself, if need be, with the anti-rabic views of Pasteur, before fighting a demoniac hydrophobe. But how can a man protect himself against the non-sentient eruptions of a mud-volcano? Let the mud fall down again naturally on the mouth of the crater! This is the way of mockers, and, in such cases, of wise men. As regards naturalists, curious by temperament and charitable by duty, they examine the pathological side of a case, and discover therein reasons for indulgence.

For some time, the dictatorial manner of the volcanic doctor, his magniloquence, his vicious habit of conducting himself like a "grande puissance traitant de pair à pair avec l'universalité des autres botanistes" (Spitzer), his explosive susceptibility, have appeared alarming signs. The suspicion that he had a bee in his bonnet was supported by another symptom, nearly always the concomitant of ideas of grandeur: the fixed idea of persecution. In fact, in several notes in his book, of which one is entitled *Falsum infamans Aschersonii*, Kuntze relates how Paul Ascherson has been persecuting him (*verfolgt*) from time immemorial. Here are the facts (*Rev. g. pl.* iii. note 84): At a flower show, got up by twenty botanists of Brandenburg, a competition was held as to who could find the most novelties. But the victor's palm, instead of falling to Kuntze, was "insidiously" disputed by Ascherson. This little story goes back to 1865. Kuntze adds that he was by no means ambitious for the victor's palm. It is on

that account, perhaps, that he has never stomached this first "persecution." We note that the "*falsum infamans Aschersonii*" treats of a very minute question of —— consonants. M. Ascherson had hazarded the opinion that, according to the new Kuntzean rules of orthographic licence, certain assonances, as *alpinum* and *albinum*, were liable to cause the rejection of one or other of the two names. Thereupon, blue fury of Kuntze. It was, according to him, more than an insult, it was a falsification covering him with infamy.

One sees the morbid outrageous disproportion between action and reaction. Reflex actions in Kuntze are enormous. A mere nothing upsets them, just as it is quite enough to tickle the epidermis of a strychnined frog to put it in a state of opisthotonos. Dr. Kuntze is normally, perennially strychnined. His inhibitor centres have forgotten to allay the effects of ascending excitation; as I say, he has lost his moderator nerves. *Albinum* for *alpinum* makes him writhe like a mountebank; his anger cannot contain itself, and he has no respect for the hardly closed tomb of M. Alphonse de Candolle. Let us quote him (without correcting his misprints):—

"Les notes suivantes etaient finies avant la mort d'Alph. de Candolle; jè préfère de les publier sans aucune changement. (O. Kuntze, *Rev.* iii. note 20.) C'est une arrogance ridicule! On a déjà accepté en part (*sic*) de mes noms nouveaux dans plusieurs livres botanique des deux hémisphéres. Que voulez donc M. D. C.? (*sic*). La plus grande quantité des noms que vous croyez erronément synonymes sont le résultat des 'Lois.' *Et moi je connais ces lois*, comme vous verrez dans mes autres notes, *mieux que vous-même* [c'est nous qui soulignons]. Vous les avez oubliées et vous faites des lois nouvelles fantastiques que vous croyez loyales (*sic*) parce qu'elles viennent de vous. Mais après la modification et l'acception (*sic*) de vos propositions par le Congrès, elles ne sont pas plus les vôtres (*sic*); elles sont devenu les Lois et vous avez à leur obéir aussi bien que tout autre citoyen."

The police at once!

The inverse case is found in impulsives when their *amour propre* is agreeably tickled. Kuntze puts first, in his puffs, a statement of M. G. Poirault, in which he has detected some commendation. Let us judge of the commendation:—

"Ainsi, pour différentes raisons, et par des procédès divers, M. O. Kuntze est arrivé à changer 1074 noms de genre et de ce fait c'est environ *trente mille* plantes qui reçoivent des noms nouveaux. *C'est beaucoup mais il paraît que c'est au plus juste* [c'est Kuntze qui souligne!!]" et plus haut, "*Urbanisol* O. K. (remplaçant *Tithonia* Desf.) a je ne sais quel reflet d'Orient . . . et, si des noms comme *Schweinfurthafra* O. K., *Müllerama* O. K., *Hasskarlinda* O. K., avaient chance d'être adoptés, on verrait de suite que le premier de ces genres est africain, le second américain, le troisième spécial à la péninsule indienne. M. Kuntze excelle à arranger les choses le plus simplement et le plus

brièvement possible.—Il est à souhaiter *qu'on oublie le novateur* [c'est nous qui soulignons] pour donner du monographe toute la considération à laquelle il a droit." (*Journ. de Botanique*, Mars, 1892.)

This translucent panegyric has been swallowed whole by the candid doctor, who, clumsily and lamentably, has fallen into the trap set for his immodesty. After that, it is quite fitting for him to call others "imbeciles," "idiots," and "sheepsheads." Need we have another example of the cock-and-bull stories, and the laughable illogicisms into which impulsives of Kuntze's kidney fall without suspecting it?

"M. Levier," writes this hot-headed doctor (*Rev. g. pl.* iii. p. 51), "me provoque dans sa *Rectification* (*Bull. Herb. Boiss.* 1896, p. 575) au duel littéraire, mais on ne se bat pas contre des idiots, on les traite tellement qu'ils ne puissent plus faire de mal, ce qu'il est de mon devoir de fair maintenant." (Listen to the method) "Il m'a donc autorisé à l'appeler Schafskopf, conformément il a donné plus tard ses jugements avec une infaillibilité soubite (*sic*) sur beaucoup de question de la nomenclature sans en avoir la capacité, tout en usant de sa verbosité inquiétante et de sa fantaisie; mais, en considération de l'arrogance importune de ce Schafskopf et en considération de son artifice et de la fausseté extraordinaire qu'il a prouvées plusieurs fois, il ne serait point injuste de le stigmatiser comme *charlatan dangereux*."

After this thrust, one would perhaps think me dead, and my evil-doings cut short. Not the least in the world. In his picturesque gibberish Otto Kuntze goes on:—

"Il (Levier) est aussi dangereux, parce qu'il va empêcher un Congrès compétent pour s'épargner la correction de sa régle à part (*sic*) et l'application de ses idées fixes."

Very good. I can rise up, with all the holes drilled me, in the omnipotence of a demi-god, having all the botanists of the world in my pocket! Kuntze, decidedly, has no chance with his "Sheepshead." His sharpest arrows return cruelly upon himself, and he *sheepsheads* himself vigorously. But a truce to gaiety; let us neglect even this neologism: auto-sheepsheadification, worthy to be by the side of *Biscogneauxia* O. K., of *Müllerama* O. K., of *Peckifungus* O. K., etc., and—courtesy for courtesy—let us compliment Jupiter O. K., on his double-escapement thunderbolts, good for killing, and better for restoring to life.

The energy of Doctor Kuntze is inexhaustible; the manifold talents that he displays to make laughter at his own expense, appear as a diamond of a thousand facets which each throws its own bright flash. Never short of burlesque and acrobatic inventions, after having committed *Koehneago*, *Lippomuellera* (blear-eyed Mueller), *Cesiusia*, *Pasaccardoa*, *Radlkoferotoma*, and other quips, the imaginative doctor has searched further and found better. He has surpassed himself. The plum of his pie (after *Tripocorynelia tripos* O. K.) will without contradiction be the "*Trifolium charlatanicum*, avec les trois variétés *levierianum*, *lejolisianum*, *aschersonianum*—les types!—de plus nov.

var. ? *malinvaudianum*, véritables herbes mauvaises qui pulluleront à Paris en 1900."—"Les malheureux! Voilà ce qu'ils font du langage des fleurs!" observes, *à propos* of this trump card, the *Journal des Débats*, 14th October. *Et nunc erudimini.*

Have we been sufficiently absurd, O my learned friends, to take literally, for six years past, the rhodomontade of this old bogie, eater of botanists. His savage roarings, his round matador eyes, the dreadful twirls of his cabbage-cutter, hid a humorous and sportive nature. Like the street mountebanks who pretend to swallow snakes and swords. Scratch the sword-swallower and you will see revealed the chalked nose of the clown so dear to infancy.

But our Old Bogie is not comfortable. Every time he conjures up the menacing spectre of the 1900 Congress, he beholds the repropagation of the tare that his claws have just destroyed and he loses the thread of his ideas. An irritating vision haunts him—that of an assembly where one will demand better arguments than "pickpocket," "bully," "Jesuit," and "sheepshead," and where turbulent humbugs, demoniacs, retailers of pamphlets at twenty-eight marks the volume, will be summoned back to good manners. The "Trifolium" proves satisfactorily that Doctor Kuntze has still lucid intervals.

Not for the sake of Kuntze, but for those who may have succeeded in construing his gibberish into current language, I have brought to light one, or rather two calumnious assertions upon which this pure flower of learning returns with a fury and with hiccups of big words (fifteen pages of abuse) that denote a bad conscience.

He does not cease to write and to print that I have abominably violated a "contract" between us on the subject of our private correspondence. Now there is a unilateral contract and a bilateral one. To request some one whom you suppose to be a gentleman to hold as confidential a correspondence that you commence, is not a contract; it is a courteous appeal to the elementary rules of honour. M. Kuntze, on the other hand, considers a "contract" the following procedure: to accept the correspondence, to carry it on to the nineteenth letter, and then to break it off abruptly, with this ultimatum: "*Je vous défends de publier quoi que ce soit en fait de nomenclature, si vous ne faites pas paraître en même temps et in extenso toutes mes lettres* **et toutes les vôtres**" (this last part of the paragraph is adroitly suppressed in the *Revisio*, iii. (2), p. 58, where there is only a question of "reciprocity"). M. Kuntze having a lively interest in forcing my silence—one sees why—and in stifling my objections, found it quite natural to shield himself behind the privacy of my letters, and threatened me, in no equivocal terms, to divulge them in case of disobedience. This trap he decorates with the pompous title of "contract." Readers of the *Bulletin de l'Herbier Boissier* (1896, p. 575) know in what very categorical terms I have rejected this fool's contract, terms which have not prevented Kuntze from republishing the lying

assertion that I accepted it. But Kuntze has raised to an academical art the art of making authors say things that they did not say. I congratulate myself to-day that I did not shrink from the publication of some phrases torn from my early letters. They prove that M. Kuntze deludes himself and wishes to delude others in attributing to me hostile intentions from the beginning of our epistolatory relations. When one attacks in treachery and in secrecy (*hinterlistig aushorchen*), one weighs one's words somewhat differently. To have reduced an adversary short of arguments to this poor military device, to have put him in the shameful necessity of raking up confidential letters, keen sometimes but never uncivil, to respond to public criticism, to see him struggle like a madman in his virtuous rôle of slaughtered lamb, all for words, for halting names (noms à béquilles), for priorities more or less shadowy,—such a result is to encourage a third year's candidate in nomenclature. I do not grudge Otto Kuntze a real obligation for having guided my first steps. One is not born a nomenclaturist, as one is born a cook-shop keeper. The eloquence of his eighteen letters made me a model disciple, and his comminatory ultimatum has given me the heroic courage (Todesverachtung), which I should never have otherwise found, to bring before the public our interesting little discussions. Tu l'as voula, Georges Dandin!

The second folly of M. Kuntze consists in unwearyingly and everlastingly repeating that I have suppressed in my French translation of the Berlin report, made in great haste at Bormio in the presence of Professor Ascherson, who took it away to Genoa, the judgment of Viennese botanists, *favourable to the reform of Kuntze* and *unfavourable to resolution* vi. *of Berlin.* (81 Generic names debarred; among them: *Mokof, Brami, Oureti, Gansbium, Hondbessen, Chocho, Belutta-Kaka* Ad.)

M. Kuntze pretends to be ignorant of, or believes the public sufficiently badly informed to be ignorant of, the following passage from *Atti del congresso botanico internazionale di Genova,* 1892, p. 104, line 6, below: "Un groupe de botanistes autrichiens, réunis sous les auspices du professeur v. Kerner *condamne notre projet* (iv.) au nom de la loi inconditionnelle de priorité, et tel paraît également le point de vue de M. Müller, Arg." (Rapport de M. Ascherson.) Is this clear enough? Where is the suppression of the *unfavourable* verdict? Does M. Kuntze now desire me to translate for the French public, passages from the Viennese document that he thinks *favourable* to himself? Here is the beginning:

"Les soussignés ayant pris connaissance d'une lettre de M. Ascherson à M. v. Kerner déclarent saluer avec plaisir l'initiative d'une démarche tendant, d'une part, à enrayer le *confusion causée dans la nomenclature botanique par l'ouvrage récent de M. O. Kuntze, Revisio generum plantarum,* d'autre part, à compléter les lois de la nomenclature. Les soussignés se saillirent sans réserve (*vollinhaltlich*) aux résolutions

ci-après (I, II, III)[1] et s'engagent, dans les questions de nomenclature qu'ils auront à traiter, à mettre en pratique les principes qui y sont formulés."

These three resolutions annihilate almost all the innovations and all the *nobis* of Kuntze, since they repudiate his starting-point of 1735, his *nomina semi-nuda* and the greater part of his rules for orthographic licence. All the end of the report says regarding custom, stability of names, etc. is borrowed from Articles 3 and 4 of the Paris Code that was not invented by Kuntze, and does not pay any compliment to his skill. Dare I say *more Kuntzeano*, that the gloomy doctor possesses "un véritable génie pour tordre et défigurer les choses les plus simples"? God forbid! I confine myself to noting the black ingratitude shown toward M. Ascherson, who, before the congressionists of Genoa, wished to draw a veil over the disastrous appreciation of the *Revisio* by the Viennese botanists, and relieved me from making any translation. But M. Kuntze dotes on eulogies and treats as *compères filous* (*sic*) those who hide them from the world. He finds eulogies in the Viennese reports as he finds them in M. Poirault's bibliographies. It is a little weakness that one must allow to great men.

And, *à propos* of great men, granted the prodigious, the phenomenal aptitude for work of which Otto Kuntze has given us such startling proof, is it not a pity that this force should expend itself on barren argument over words and names? Who can M. Kuntze hope to convince with his intemperate language? Is it not better to persuade than to abuse? Once more, it is a disease, and M. Kuntze gives fair game to his contradictors (pardon! to his persecutors), by lightheartedly "suiciding" his *Revisio*, which contains *passim* some excellent things. He must take care of his temper, improve his naughty humour.

The atrabiliar temperament, complicated with ideas of persecution, only demands attentive hygiene, exercise, fresh air, a cold bath on rising, not massage but gentle aperients, very little spice and nomenclature, much bicycle, a bit of fishing with a worm, walks (vasculum on back) through fields and woods. Twenty-five thousand hours' sedentary hard work, for seven years, to make columns and columns, with the aid of dictionaries, of names, *nuda* and *semi-nuda*, and accepted, and to blow a gigantic soap-bubble which the first critical pin-prick bursts piteously—what health could resist it? And this, properly speaking, is not botany.

"Botany," says a worthy American colleague, "has for its aims the study of plants, and not that of their labels." Botany is a healthy and delightful occupation—it softens manners. Nomenclature, in the method of Kuntze, reduces them to savagery. This parasitic science,

[1] And not *sous réserve des principes* (*unter principiellem Vorbehalt*) as Kuntze falsely quoted in *Rev. g. pl.* iii. (2), p. 67.

this *Pulex irritans* of botany, tends to bring upon us the laughter of the public. If we cut one another's throats over *Chocho, Gansbium, Belutta-Pipi, Peckifungus, Biscoigneauxia,* or *Bisaschersonia,* our reputation as scientific or serious men is gone. "Féroces comme Vadius et vaniteux comme Trissotin," is already scored to our account *à propos* of "Trifolium." We must put the drag on.

Study and flowers! especially flowers!

[Then follows "Note supplémentaire (altérations grossières du code de Paris par Otto Kuntze," but these being pure technicalities we must refer specialists to the original pamphlet, which, though it is not stated, seems to be an extract from some periodical.]

Regeneration: Facts and Interpretations.

By Professor August Weismann.

My original intention in this essay [1] was simply to give an account of some observations, recently published but probably not yet widely known, which seemed to me to throw fresh light on the phenomena of regeneration. It became necessary, however, to interweave with the narrative some general remarks, in order to show on the one hand how these and other additions to the data of our science harmonise with the views I have previously developed, and on the other how these views must be modified in order to bring them into accord with the facts of the case, as we now know them.

We will first consider experiments made by Edmond Bordage,[2] Director of the Natural History Museum on the island of Réunion (Bourbon), with regard to the regeneration of the beak in birds. These deserve general interest, since they show that what has hitherto been regarded as a rare exception is really the rule, and since thus one argument against the interpretation of regeneration as an adaptive phenomenon falls to the ground.

In supporting this interpretation of regeneration I have previously [3] expressed the opinion that the regenerative power of a part is to be considered, not as a direct and necessary expression of the nature of the organism, but rather as a capability which, though it may be absent, is found wherever it is necessary in the interests of species-preservation. The capability of a part for regeneration seemed to me to depend, *ceteris paribus*, on whether the part was frequently liable to be lost in the ordinary course of life, and also on its relative biological importance for the animal. The weak and almost rudimentary limbs of *Siren* and *Proteus*, which swim in snake-like fashion, are not replaced

[1] Translation revised by Prof. W. N. Parker.

[2] *Compt. Rend. Soc. Biol.* Paris, July 9, 1898.

[3] "Das Keimplasma, eine Theorie der Vererbung," Jena, 1892. (English Ed., "The Germ-Plasm, a Theory of Heredity," London, 1893.) The idea there elaborated had been previously more or less distinctly suggested from various quarters. Thus even Réaumur pointed out that the power of regeneration was especially characteristic of animals whose brittle body was frequently liable to risk of breakage, and also of those, like earthworms, which are liable to be partially devoured by others ("Mem. pour servir à l'hist. insectes," Paris, 1738). In modern times, Lessona and Darwin have developed the same idea.

when amputated, but the reverse is true in regard to the gills, which, in these forms, are not infrequently bitten off. The latter are physiologically important organs, but this can no longer be said of the former. On the other hand, the strong and biologically indispensable limbs of the less elongated newt (*Triton*) have a high regenerative capacity.

The fact that internal organs not naturally exposed to mutilation are not regenerated in animals which show great regenerative power as regards external structures is in harmony with this view. Halved lungs in *Triton* do not grow again, but simply close up, and I can now add to this the fact that the oviduct and the vas deferens are neither renewed nor even elongated by fresh growth if a portion of them be excised. The experience of pathologists is also in agreement with this, for they report that they have never seen the regeneration of an experimentally excised lobe of liver or kidney in any animal, but only an increased function in the remainder of the organ—in short, there is no morphological regeneration but only a physiological compensation. On the authority of my renowned colleague in pathological anatomy, Ernst Ziegler,[1] I may add that, in man, the power of regeneration is possessed only by such tissues and portions of tissues as are subject, in the normal course of life, to continual or periodical wearing out. Such are, the mucous membrane of the intestinal tract, the epidermis of the skin, the whole mucous membrane of the uterus with its glands, etc. To these must be added certain tissues which are very frequently but not regularly or periodically liable to injury, and are at the same time of great biological importance. To this category the cutis belongs. In it, however, as is well known, complete histological renewal does not take place, but only a partial reparation by means of cicatrisation. The scar-tissue—which is also formed from connective tissue, but contains no skin-glands, and has a different arrangement of the connective-tissue strands—is well adapted to the closing of small skin wounds, and quite sufficient to re-establish in approximate integrity the important protective adjustments of the cutis. Regeneration never occurs, on the other hand, either in nerve-cells of any kind whatever, or in the supporting (neuroglia) cells of the brain, notwithstanding their great functional importance. All these facts are in harmony with the theory suggested, that the power of regeneration possessed by an animal or a part of an animal is regulated by adaptation to the frequency of loss, and to the extent of the damage caused by the loss. Hitherto, however, it seemed impossible to reconcile with this proposition the case communicated by Kennel,[2] to which I have already elsewhere called attention,[3] of the "stork the

[1] E. Ziegler, "Lehrbuch der allgemeinen Pathologie und der pathologischen Anatomie," Jena 1895. But the above statements are based on an unpublished lecture delivered by Ziegler in 1898 before the "Naturforschende Gesellschaft" in Freiburg-i-Br.

[2] Kennel, "Ueber Theilung und Knospung der Thiere," p. 18. Dorpat, 1888.

[3] "Keimplasma." (Eng. Ed. p. 125.)

upper beak of which was accidentally broken off in the middle, and the lower one being sawn off to the same length, both were completely regenerated."

Until quite recently there seemed no warrant for the assumption that the breaking of the beak in birds was of frequent occurrence, as no other similar observations were recorded. This case, therefore, seemed to indicate that the capacity for regeneration in a part does not depend on special adaptation to a great liability to loss, combined with high biological value of the organ, but rather that it is due to a general adaptation,—to a power of regeneration possessed by the whole organism which, to a certain extent, may come into operation in any part in which loss occurs, even though the loss takes place quite exceptionally.

This apparent contradiction of the adaptation-theory of regeneration is now set aside by the observations of Bordage, who observed that, in the island of Réunion, where the sport of cock-fighting is popular, injuries to the beaks of the cocks are of frequent occurrence, and are regularly followed by regeneration. Bordage found that, as the result of a fight, the beaks of the cocks were often damaged, but that afterwards complete regeneration took place. The injuries were partly inflicted by blows with the beak, partly by "un terrible coup de patte," but they usually affected at most only the terminal third of one or both jaws,—"that part of the upper mandible which represents the premaxillae or unpaired inter-maxillary resulting from the fusion of the premaxillae, and of the lower mandible the triangular portion formed by the fusion of the two bones at their extremity." These parts of the beak may be entirely broken off, when regeneration takes place, and both the bones and their horny covering are renewed.

In rare cases, the injuries to the lower mandible are so great that it is broken and hangs down, and has to be artificially fixed in its place. In such a case, of course, the animal cannot pick up its food, and has to be artificially fed, for the regeneration of the lower mandible requires two or three months.

Although these observations refer only to cock-fighting instigated by man, they have, as the author rightly points out, a wider bearing. It is well known that the males of numerous birds fight fiercely with one another in the breeding-season, the beak being naturally the chief weapon, and that this is true of storks was known to Brehm, who says that jealousy often leads to fatal combats.

The case described by Kennel can therefore no longer be regarded as evidence against the view of the adaptive nature of regeneration, and thus the one observation which gave ground for referring regeneration in a highly-differentiated animal to a *general* power of regeneration loses its force. In recent years, it is true, the renewal of the artificially removed lens of the eye in *Triton* has been cited in this connection; but this is hardly justifiable. I, at least, must own to being surprised

that the argument based on this case remained unchallenged. From Calucci's experiment of extracting the lens in *Triton*, which resulted, as was expected, in regeneration, G. Wolff[1] inferred that there was here "a new adaptiveness appearing for the first time," since it is "impossible that regeneration can take place in accordance with the inherited mode of its ontogenetic origin." The latter conclusion is indeed correct, but the former one does not follow from it, and it seems to me quite out of the question to bring forward this case as contradictory to the view that regeneration depends upon a special inherited adaptation. It will, of course, be readily admitted that newts, in their natural conditions of life, are not liable to excision of the lens, but it does not necessarily follow that the lens in these amphibians cannot be adapted for regeneration, for at any rate it may be lost along with other portions of the eye. As far back as 1781, Bonnet and Blumenbach showed that the eye of newts would renew itself completely even when all of it except a small portion (according to Blumenbach, as little as one-fifth of the bulb) was extirpated, and the more recent observations of Philippeaux (1880) have confirmed these conclusions. This regeneration certainly appears to us especially remarkable, since it necessarily presupposes quite a different mode of formation of the eye from that which takes place in the embryo. Leaving out of account altogether the question as to whether we are or are not in a position to form a theoretical conception of the phyletic origin and the nature of this process, it is at any rate certain that some sort of mechanism for the regeneration of the eye exists. But if this is true *for the whole eye*, why should we be surprised that a *part* of it, the lens alone, may be regenerated. A single toe of the newt's foot, if cut off, will grow again just as well as a whole leg.

There is no doubt, however, that the eye of *Triton* is susceptible of injury, and that it is frequently injured in natural conditions. Even newts fight among themselves, and I have elsewhere shown[2] that these animals, at anyrate in the breeding-season, ferociously attack and injure one another. I once put a number of newts together for a short time in a small empty glass, and I then saw that the animals attacked each other furiously, biting and struggling pertinaciously. "Several times one seized another by the lower jaw and tugged at it so violently that it would have been torn out if I had not forcibly separated the animals." From this I concluded that "the loss of part of a jaw or eye may therefore not infrequently occur in the natural state," and I may now add that the newt is liable to attacks by water-beetles of the family Dytiscidae. I was for a time uncertain whether the powerful poison which newts, like toads, secrete from the skin was any protection against such attacks, but this is not the case. Large Dytiscidae attack the newts whenever they can find no other food, and they eat away

[1] *Arch. Entwickelungsmechanik*, Bd. i. p. 380.
[2] "Keimplasma," p. 167. (Eng. Ed. p. 125.)

their flesh to such an extent that the newts die. With small Dytiscidae I have as yet made no experiments, but it is unlikely that the poison serves as a deterrent to them either, and, since they are unable to kill the newts, they are the more likely to injure and eat away certain portions, such as the eye, with their sharp-pointed jaws. A trustworthy observer informs me that the larva of *Dytiscus marginalis* seems to go to work on a certain system, for it tries to get at the newt from above, and to attack it on the back just behind the head. Observations specially directed to the point may perhaps determine whether injuries to the eye take place. But even then we should have no *proof* that the regeneration of the eye is to be regarded as a case of adaptation of the organ to its frequently occurring mutilation. In such cases, however, formal proof is hardly attainable; it must always be a question of probabilities. Who, for instance, would attempt to determine how often the beak of the stork is injured? And, even if this were done say for ten years, over a comparatively large area of the bird's habitat, we should be confronted with the wider and unanswerable question, as to how often such injuries must occur in order that Nature may be prompted to effect, by means of selection, or in some other way, a mechanism of regeneration for the beak. But the mechanism in this case has been established, and for internal organs such as the lungs, oviduct, and vas deferens of newts it has not; there is therefore a greater probability in the conclusion that regeneration is regulated by adaptation, than in the inference, from the renewal of the lens in *Triton*, of the presence of a general power of regeneration,—"an adaptiveness not inherited but primary." It will, moreover, be apparent farther on that we have good grounds for assuming that an apparatus for regeneration once established degenerates with exceeding slowness when it has become superfluous. It is thus even conceivable that this apparatus may still persist although no longer necessary in the present conditions of the newt's life.

That such an apparatus is certainly present in this case is apparent even from the fact, in regard to which all past and present experimenters are agreed, that the eye of the newt is never regenerated if it be completely excised, and that therefore it is not any kind of cells, but definite cells belonging to the organ itself, which institute the regeneration, as is true in the regeneration of the intestinal canal in pupahood. As I showed in 1864[1] in the case of *Musca*, the intestine during this phase of life breaks down by histolysis and is immediately reconstructed. In 1888 Kowalewsky[2] and van Rees[3] were able to demonstrate in the same insect, and C. Rengel in 1896 in a beetle, that the new formation had its starting-point in special cells, fairly equally distributed throughout the mucous membrane, which did not break down during pupahood, but on the contrary increased rapidly,

[1] *Zeitschr. wiss. Zool.* 1864, Bd. xiv. [2] *Op. cit.* 1888, Bd. xlv.
[3] *Zool. Jahrb.* 1888, iii.

and gave rise to the new mucous membrane. These "primitive cells of the epithelium of the mid-gut in the imago," as Rengel[1] calls them, thus form in this case the distinctly demonstrable regeneration-apparatus of the epithelial wall, and we may assume the existence of a similar regeneration-apparatus in the eye of the newt.

The results of an investigation bearing specially on the question of the adaptive nature of regeneration have been recently published by T. H. Morgan,[2] and these results are undoubtedly of value, although I find myself unable to agree with his interpretation of them. He selected the hermit-crab (*Pagurus*) as the subject of his investigations, and this was certainly a very suitable choice, since the experimenter has here at his disposal a series of appendages which, on the one hand, are very different in their biological value, and on the other are exposed to the danger of loss in widely varying degrees. Examination of freshly-caught animals showed that, out of a hundred hermit-crabs, nine lacked one of the three anterior legs, while none were without the small fourth and fifth, which probably serve to fix and support the animal within its gasteropod-shell. This result is only what might be expected when we bear in mind that the hermit-crab withdraws into its dwelling-place with great rapidity when danger threatens, and that the fourth and fifth legs must be the first to reach the shelter of the shell. In any case, these limbs are unlikely ever to be the aim of an attack, which is probably always directed against the three larger and more exposed limbs in front of them. As the posterior part of the body remains entirely within the shell, the abdominal appendages are perfectly protected, and could be injured only during a change of dwelling, or if an attempt is made to draw the animal forcibly out of its shelter. Accordingly Morgan found that only one out of a hundred specimens lacked the second or third abdominal appendage, and there is still the possibility that this defect was congenital, and not due to injury. The problem these experiments were intended to solve was this: *Is the power of regeneration graduated according to the probability of loss?* Are those limbs most easily and most frequently regenerated which are most frequently injured? and are those which are never injured, perhaps never regenerated at all?

Experiments in cutting off the various limbs showed that they were all capable of regeneration, though they did not all grow again equally often, the anterior abdominal appendages renewing themselves less frequently than the thoracic limbs, though even these did not become renewed in every instance. I need not go into further details; it is sufficient, as far as the main question is concerned, to know that all the limbs possess the power of regeneration, those most liable to injury and those naturally well protected alike. The relative biological importance of the different limbs in *Pagurus*, too, does not seem to be

[1] *Zeitschr. wiss. Zool.* 1896, Bd. lxxii.

[2] "Regeneration and liability to injury," *Zool. Bull.* Boston, 1898, vol. i. No. 6.

associated with any difference in the capacity for regeneration, since the almost rudimentary first pair of appendages on the abdomen of the male were renewed quite as often as those on the abdomen of the female, although these last fulfil the important function of egg-carrying.

These facts appear to tell very decidedly against the conception of regeneration as a capacity regulated by adaptation, and the author interprets his results in this way, and sums up as follows: "*There is no relation between the frequency of loss and the regenerative capacity of a part:* those who believe that there is such a relation overlook an important part of the problem. Even if it had been found that the parts most susceptible of injury were those most capable of regeneration, it would not follow that this depended on so-called Natural Selection. To assume this is to overlook the fact that unless these animals had had from the beginning the power of replacing lost parts they could not have survived at all, but would of necessity have become extinct."[1]

These are astonishing views. One might just as well say that it is impossible for Natural Selection to have brought about any change in an important part or character in the course of phylogeny, because the particular variation necessary to life must have been present from the first, since otherwise the species must have died out! It seems not to have occurred to Morgan that the changes in the structure of a species may have kept pace with the changes in the conditions of its life—yet this is a presupposition of the hypothesis of Natural Selection, and is indeed its *conditio sine qua non.* Hermit-crabs have certainly possessed the power of regeneration "from the beginning," but may they not have inherited it from their ancestors, the long-tailed forms, which possess it to this day and have need of it for all their appendages since all are liable to injury? And cannot, nay, must not, these in their turn have inherited it from their ancestors, the sessile-eyed crustaceans, and so on through the whole crustacean-pedigree back to the unknown annelid-like ancestors of the class? But doubtless even these possessed in a certain degree—probably even in a higher degree—the power of regeneration, from which that of the oldest crustaceans must have originated, and become localised and transformed. We know that the lower worms have quite as high a regenerative power, extending to all their parts, as the lower Coelentera or polypes for instance. It seems almost as if Morgan ascribed to me the view that the capacity for regeneration must be built up anew for each species—must be inscribed so to speak on a *tabula rasa:* my view, however, is that here, as in all transformations, Nature started with what was already present, and by modifying it brought about adaptation to new conditions. The assumed general power of regeneration in the lowly ancestors of the crustaceans

[1] This quotation is translated from the German. We have been unable, at the moment, to procure a copy of Mr. Morgan's paper.—[TR.]

would thus gradually have adapted itself to the changes in the body, and to the new conditions resulting from these changes as well as from other causes; it would have become localised and specialised. In these animals no portion of the body can now give rise to a whole animal, but the parts most exposed to injury, the appendages, have retained the transmitted power of regeneration, and localised it in certain parts, and in relation to definite stimuli. As in the course of time the appendages of the different body-segments became more widely differentiated in adaptation to different functions—giving rise to antennae, jaws, walking-legs, or swimmerets—the predisposition to regeneration in certain parts of the body slowly varied also; and thus, not indeed at the same rate, but not lagging very far behind, the adaptation of the capacity for regeneration followed the adaptation of a limb to a new function.

Any one who has followed me in my attempt to formulate a theory of regeneration will admit that all this thoroughly agrees with the principles on which it is based, and will also see that it harmonises with the theory of Natural Selection much better than any conception of regeneration which has been or could be brought forward. It is clear, for instance, that the variation of the regeneration-"Anlage" must always take place *much more slowly* than that of the part itself—in this case the appendage. For since Natural Selection consists in a sifting out of the most fit, the rapidity with which it secures its result—namely, transformation—must depend, *ceteris paribus*, on the number of individuals of a species which are selected in relation to the part in process of transformation. If in a species of which one million individuals are living at the same time, nine-tenths accidentally perish, there will remain only 100,000 for the selection of the 1000 which we will assume to form the normal strength of the species. The greater the number among these 100,000 possessing the favourable variation, the higher will be the number of the normally surviving 1000 possessing it, and the more rapid will be the progress of the favourable variation.

If, however, it be a question of the variation of a regeneration-"Anlage," selection of its favourable variations will not operate among the 100,000 individuals which chance has spared, but only among those of them which, in the course of their life, lose the limb in question and thus are in the position of having to renew it, well or ill. Assuming that this is the case with ten per cent, selection towards the improvement of the regeneration-apparatus would only operate upon 1000 individuals, and the process of transformation of the regeneration-"Anlage" would go forward much more slowly than that of the transformation of the limb itself.

Another fact must be noticed in the same connection. It has already been observed in a series of cases, and would probably be found to occur in many more if thorough investigation were made for

the purpose, that appendages are sometimes regenerated, not in the present modern form but, in all probability, on an older type. In fact, reversion to an ancestral type occurs: *i.e.* the regeneration-"Anlage" has not yet quite reached the same stage as the variation of the part itself. Thus the appendages of *Blatta*, although growing again satisfactorily, are then, according to Bateson, provided with a four-jointed instead of a five-jointed tarsus, and the same thing has been observed by Bordage in the Phasmidae. Fritz Müller also records that the long-clawed forceps of a Brazilian shrimp, *Atyoida potimirim*, is replaced by the older short-fingered type of forceps, similar to that of the allied genus *Caridina*. Such cases might, of course, be regarded as showing an insufficiency of regenerative power to replace the lost limb perfectly; but Barfurth has shown that on removal of the four-fingered fore-limb of the axolotl a five-fingered atavistic limb grows in its place. In face of such facts the hypothesis of a general power of regeneration, which restores the damaged "life crystal" to its integrity, is obviously untenable. Here the "crystal" is restored after an older pattern, and one cannot see why this should be so, since the whole has been no more altered than a dodecahedral crystal would be if the point were broken off. If, in that case, the missing portion were suddenly to be re-crystallised on another "system," it could only be because the elements from which the restoration started were, not the remaining whole, but particular parts belonging to another "system," or—to translate into the language of biology—because at the injured places there were regeneration-"Anlagen" of earlier origin which came into play as the result of the injury. The existence of "Anlagen" is thereby proved.

It has been said that to explain such facts by reversion is to give no explanation at all; and that such an interpretation, though resulting in ranging the facts under a definite category of phenomena, in no way elucidates the cause which, after the loss of an eye, induces the appearance "of a different type of ancestral-organ. This, however, is just what we want to know" (Herbst). For my own part, I think it is better not to want to know too much all at once. It may be that some day we shall attain to a knowledge of the actual physico-chemical processes which condition development; but, in the meantime, we are many times the distance to Sirius short of that knowledge, and we shall do well to content ourselves with first striving after nearer goals. Even the simple recognition that we have here to do with a case of atavism—that is, of reversion to an ancestral form—is a creditable piece of knowledge, since it at least enables us to understand why the lost eye of the crab is not replaced by, say, a tuft of feathers, or a monkey's tail. Perhaps, however, the idea here expressed, that the regeneration-"Anlage" lags behind the phyletic transformation of the part itself, may carry us a step farther, always presupposing that we do not regard the principle of Natural Selection as "the greatest error of the century" (Driesch).

Let us now return to Morgan's results with the hermit-crab, in order to attempt an explanation of the fact that all the appendages of this animal have remained capable of regeneration—even those which are protected by the shell in which it lives, as well as the abdominal appendages of the male, which are presumably rudimentary and have no function to fulfil.

The cause of this seems to me to be the same as in the case I have just discussed, viz. the lagging of the regeneration-"Anlage" behind the progressive transformation of the limb—the fact that variation, even if it be retrogressive, goes on much more slowly in the regeneration-"Anlage" than in the limb itself. The predisposition to regeneration is useless in the case of limbs which are completely concealed and therefore not liable to injury, as well as in the case of non-functional appendages: but it cannot be in any way harmful, and therefore will not be gradually got rid of by an active negative process of selection—as are the abdominal appendages on the right side which are directed towards the axis of the spiral shell—but by that slow process of disappearance of parts which have become useless but are not harmful, which I refer to the operation of germinal selection under the influence of Panmixia. I hope to discuss this process more fully elsewhere, and, for our present purpose, it is not essential to have before us a clear theoretical statement of the gradual degeneration of useless parts and of the regeneration-"Anlagen." If it be certain that this gradual down-sinking and degeneration takes place *with extreme slowness*, we need not wonder that the predisposition to regeneration, which was for ages indispensable to the life of the animals, should still persist long after it has become useless and superfluous. From a phylogenetic point of view, we are justified in saying that the whole group of hermit-crabs came into existence only recently, that is, since the end of the Cretaceous period. In any case, we know that a species of Urodele (*Proteus*) has lived an incomparably longer time in the caves of Krain, and we need not wonder that in this hermitage it has lost the power of regeneration in limbs and tail, while the hermit-crabs still possess it in their abdominal appendages.

There is yet another group of phenomena which stamp the power of regeneration as an adaptation on the basis of inherited preliminary conditions, viz. the self-amputation of limbs, a process which has repeatedly been studied of late years. This was described by M'Culloch in 1826, later by Goodsir, and in recent years by Frédéricq and others. Bordage has also written about the so-called "autotomy" of leaf-insects (Phasmidae), in regard to which he established the fact, mentioned above, that young specimens of *Monandroptera inuncans* renew the tarsi after amputation with only four joints instead of five. Bordage[1] has taken advantage of his recent sojourn on the island of Réunion to

[1] "Sur les localisations des surfaces de régénération chez les Phasmides," *Compt. Rend. Soc. Biol.* Paris, 1898.

investigate the process of regeneration in the Phasmidae more fully, and he found that artificially-caused defects were made good only from particular regions. Regeneration from the cut end takes place only if the tarsus or the lower third of the tibia be cut off. If the limb be cut through higher up, the remaining portion of the leg is thrown off, and regeneration starts from the point of fusion of the trochanter with the femur, these parts not being connected by a joint as in other insects, but forming a thin suture at their union, and it is there that the breaking off of the limb takes place. The same state of things prevails among crabs and shrimps, in which M'Culloch has described the mechanism which brings about self-amputation of a limb when certain stimuli, such as compression or amputation, are brought to bear on it. At this point regeneration takes place, and here, therefore, the power of regeneration is localised. Both the capacity for autotomy and the power of regeneration limited to certain definite regions obviously imply not "primary adaptiveness" (*Zweckmässigkeiten*), but secondary arrangements, adaptations to perfectly definite and special conditions.

In order to learn something of these conditions in the case of the Phasmidae, Bordage instituted various experiments. He placed the insects with some birds, and observed that the latter—as indeed was to be expected—never merely injure the insects but invariably kill them outright by pecking at them with their bills. This is particularly true of the king-fisher of that region, *Acridotheres tristis*, which is a great destroyer of the grasshoppers and Phasmidae.

Birds, therefore, are never the cause of autotomy and regeneration of a limb; they are simply among the eliminators of these insects, not among the moulders of their regeneration-arrangements. But it is otherwise with small lizards. These (*e.g.*, *Calotes versicolor*) often seize the Phasmid by the body and swallow it at once, and this happens particularly often to the larva. But when an insect is full grown and its assailant is relatively small, the latter usually only succeeds in seizing its victim by one of the long legs, and then it tries to work its way gradually upwards with its mouth so as to get hold of the body itself. It does not bite very deeply, but presses the legs with just sufficient force to prevent the prey escaping, the insect meanwhile resisting by clinging convulsively to the nearest object. The stimulus supplied by the compressing teeth and the forcible pulling at the leg may cause the breaking off of the latter at the suture, and if the insect then lets itself fall from the twig on which it is sitting, it is saved. But, according to Bordage, in most cases it does not do this, but tries to run away, and is then easily recaptured and overpowered by the *Calotes*.

It happens sometimes, though not often, that the lizard catches hold of the leg by the tip only, and breaks it off, and thereupon regeneration ensues as already described, without the preliminary throwing off of the whole leg, which occurs only in response to

stimulus to the femur—when it is pinched, crushed, or cut through the middle: only in this region does autotomy occur.

Bordage states that the act of emerging from the egg may result in the loss of the tarsi and their subsequent regeneration, for it frequently happens that the insect does not succeed in freeing one of its legs from the egg-shell, which it is obliged to drag about to the great hindrance of its progress, until it is finally stopped entirely by the catching of the shell on some object. In such a case the insect struggles with all its might to free itself, and in doing so not infrequently loses the tarsus.

Something similar takes place during the process of ecdysis, for it frequently happens that the insect remains entangled in the old skin. Of a hundred Phasmids nine died in this way during moulting, while twenty-two tore themselves free with the loss of one or more legs, and only sixty-nine survived the process without loss.

Obviously, then, the power of autotomy with subsequent regeneration is here a frequently-used and advantageous arrangement; but I cannot agree with Bordage in regarding the pulling and struggling of the insect in the effort to free itself from the encumbering skin as the *directly* operating cause of the fusion of femur and trochanter which gives rise to the breaking-point adapted to autotomy. Even if this pulling and struggling lasted for a whole day, it is extremely doubtful that it could induce anchylosis of the joint; and if it could, it would be impossible that the anchylosis should be transmitted, and that it should thus form the basis of a general and transmissible fusion of the joint; for how could a variation of the joint influence the eggs or sperm-cells of the insects?

The facts communicated by Bordage are especially of value, because they show that it is not only the enemies of the insect in question that have a part to play in the development of the tendency to autotomy and regeneration, as has hitherto been thought: other events are also concerned which are of general occurrence and are repeated several times in the ordinary course of life, viz. the moultings. It is not impossible that the ecdyses of crustaceans operate in a similar way, but no precise statement on this point can be made without fresh investigations.

With regard to the phyletic origin of the adaptations in question, Bordage supposes that the capacity for regenerating the tarsus, the lower end of the tibia, and the whole leg from the suture has been derived from autotomy, which, again, he believes to have been brought about by persecution on the part of the saurians and batrachians of primeval times (Stegocephala). I prefer, on the contrary, to regard the power of regeneration as the older capacity, and autotomy as derived from it; for I doubt whether any part could regain the power of regeneration if it, or the parts from which it is phyletically descended, had once lost this power. In my opinion, therefore, the

legs of insects must have been capable of regeneration from the first, but may have entirely or partially lost this power in many groups, or have so modified it that it remained only at certain definite regions, as in the Phasmidae. Were the power of regeneration once lost it is scarcely possible that it should ever have been regained.

Autotomy, on the other hand, is an adaptation which might, phylogenetically speaking, arise anew at any time, where necessary, assuming that the existing parts were capable of varying and adapting themselves to that end. The existence of an autotomic adaptation of an analogous though not quite identical kind in insects and in crabs shows that the adaptation could take place in both classes, and the presence of such an adaptation in relation to the three anterior thoracic legs of *Pagurus*, together with its absence in relation to the fourth and fifth legs (Morgan), is further evidence of the comparatively recent introduction of autotomy and the much older origin of regeneration; for the fourth and fifth pairs of legs possess the power of regeneration.

The adaptation for autotomy once gained, the power of regeneration had of necessity to become localised; that is to say, the apparatus necessary for it had to be transferred to those parts at which alone the breaking off of the limb occurs in response to certain definite stimuli. In any case, however, both capacities are "*Einrichtungen*," that is, *adaptations of the organism to definite demands made upon it by the conditions of life*, and are not the outcome of primary qualities of the living substance. In regard to autotomy this is immediately clear because we know the reflex mechanism and the modification of the breaking-point upon which the adaptation depends, but in the case of regeneration we can in the meantime only guess at the mechanism which brings it about. That there is some special mechanism hardly admits of dispute.

In the beginning of the paper from which I have quoted above, Morgan casts a glance at my theory of regeneration as an adaptation-phenomenon, and not exactly a friendly glance either; for it seems to be the fashion nowadays among the younger investigators to look down with a certain lofty disdain upon so-called "explanations" which are based upon the selection-hypothesis. Morgan cannot believe that the chapter on regeneration in the "Germ-Plasm" will convince any one that the phenomena are "in any way" there explained, and he reaches a climax in the dictum, "it seems that the 'Natur-philosophie' is not yet dead." To that I can only answer: It is to be hoped that it is not, and that it never will be dead, for progress in our knowledge will always depend upon the philosophical treatment of known facts, since it is only in this way that we can set up new goals to guide our observations, and so find out new facts which will give us a deeper insight,—only in this way, too, can we recognise the importance of the facts which chance reveals to us. But if Morgan means by "Natur-philosophie" merely the degenerate varieties of philosophical Nature-

study as practised by Oken, Schelling, and others in the beginning of this century, one should surely recognise the difference that exists between this juggling with ideas and *a priori* interpretation and my attempts to gather all the facts together under a common point of view. The former are mere fantasies which will never establish anything, the latter are, to some extent, central points for scientific tasks which will yield to the investigator provisional assumptions to be corroborated or disproved: they may even give into his hand biological formulae or symbols which, though incapable of full explanation as yet, may with advantage be included in the consideration of certain problems.

When Oken, for instance, says "the contrasts in the solar-system between the planets and the solar bodies repeat themselves in plants and animals, and as light is the principle of movement, the animal has the power of independent movement as an advantage over the plant organisms which are fixed to the earth," no one will attempt to make anything of it, altogether apart from the fact that the *tertium comparationis* immobility, as we now know, is not peculiar either to the sun or to the plants. Again, Oken, with true prevision, stated that "everything organic has come from slime, and is nothing but slime in various forms; this primitive slime had its origin in the sea in the process of the development of the planets from inorganic material"; and then the primitive slime took the form of little vesicles, and "the whole organic world has as its basis an infinity of such vesicles." These are *a priori* interpretations of nature which by chance come very near the truth, but which, when they were put forward, could not in the smallest degree further the progress of knowledge, because no one then possessed the means of proving them right or wrong, or of deriving from them any deeper insight into the process of life. But, on the other hand, the "supposition" advanced in the "Germ-Plasm," that regeneration may be an adaptation-phenomenon and as such depends upon processes of selection, is not a final verdict but simply a hypothesis which may give a new lead for investigators to follow, as Morgan himself has done.

Conclusive results, of course, cannot always be obtained from a single series of experiments, for Nature's answer to our questions is not usually capable of only one interpretation, and criticism is often necessary in order that it may be read correctly. Thus Morgan's results do not afford a convincing refutation of my theory of the adaptive nature of regeneration; they have even, indeed, a bearing which tells decidedly in favour of my view. If the provisional conclusion that the almost rudimentary abdominal appendages are regenerated less frequently than the actively functional walking legs is corroborated, it, at any rate, will be intelligible from the standpoint of the selection-interpretation, but not from that of those who regard the organism as a crystal, and regeneration as an inevitable completion of the crystallisation. For if

no "Anlagen" be necessary to the occurrence of regeneration, there are none to disappear when regeneration becomes useless on account of the unimportance of an organ. It is true that Morgan does not state that this conclusion is reliable, and it will need a much larger number of experiments to prove it satisfactorily. A continuation of these experiments might also yield much in other directions. It would be necessary to observe the complete development of the regenerated abdominal appendages in order to be able to say definitely whether they were reproduced in the same form, or whether here too there was reversion to an older type. If the above conclusion be correct, *i.e.* if the phyletic variation of the regeneration-"Anlage" goes on more slowly than that of the transforming organ itself, we should expect, for instance, that in the regeneration of the last abdominal appendage, the present form—which serves as a holdfast—would be renewed after the pattern of the tail-fin of the Macrura. This is, of course, only a supposition, and it is perhaps more probable that in this case the regeneration-"Anlage" has followed more closely on the transformation of the organ, as it must be of importance that the holdfast should be renewed as such. Of course this argument would fall to the ground if it were shown that injury to this sixth abdominal appendage hardly ever takes place; for then there would be no reason for the progressive modification of the regeneration-"Anlage." But the statistics given are not nearly comprehensive enough to admit of any certain conclusions being drawn from them, and further experiments are much to be desired.

Let us now discuss another experiment made on frog-tadpoles, which, if it be corroborated, will be of fundamental importance to the question of regeneration. Before the first external indications of the hind-legs appeared, Esther Brynes[1] destroyed the whole region of the body-wall from which the legs grow with a hot needle, and observed that perfectly normal limbs were nevertheless formed. The experiments of Barfurth had previously shown that the legs of young tadpoles become regenerated, and had also established the fact, which seems to me very important, that this power of regeneration is rapidly lost as the tadpole grows. These experiments are quite in accordance with the theory of the localisation of the regeneration-"Anlage" at the base of the leg, since it must somehow be present at this stage. Esther Brynes's new experiment, on the other hand, tends to show that at an earlier stage there is also regeneration of the leg without any localised "Anlage," and that at this stage the limitation of the "Anlagen" to the cells of the mesoderm has not yet taken place.

The knowledge we have already acquired through the numerous and variously modified experiments with the blastomeres of the developing ovum here receives a new and very precise corroboration.

[1] On the regeneration of limbs in frogs after the extirpation of limb-rudiments," *Anat. Anzeig.* 1898, Bd. xv. No. 7.

Certain cells of the developing animal contain, to begin with, many "Anlagen," and which of these shall become active is decided by the position the cells take up in the organism, *i.e.* through the influences exerted upon them by the rest of the living parts of the animal.

All these phenomena, and also more especially that which Roux has called "post-generation," make it plain that my theory of ontogeny requires some modification. When it was put forward, two fundamental postulates for the explanation of ontogenetic differentiation occurred to me as possible—either that there is a regulated progressive breaking up of the "Anlagen"—masses contained in the germ-plasm into smaller and smaller groups; or that the collective "Anlagen" remain together in all the cells of the organism, but that each of them was responsive to a specific stimulus, which alone could excite it to activity; the former view may be described as a theory of dispersal, the latter as one of liberation. I inclined to the former, because, in face of the facts before us at that time, it seemed on the whole the more probable; and because I found myself in the position of being obliged to make a choice if I wished the applicability of my principles of heredity to be tested in relation to all aspects of the problem. I found myself in the position which the present Prime Minister of England, Lord Salisbury,[1] referred to in his Oxford address as one in which politicians found themselves often, but naturalists never—that of having to choose the lesser of two evils or uncertainties. The mere observer of facts certainly does not find himself in this position, but it is otherwise with the theorist who is confronted with the task of associating in idea the phenomena of a whole domain, filling up the gaps in the knowledge of the moment with probabilities, seizing the essentials of both, and combining them on a definite principle into a provisional theory. He must choose between greater and lesser probabilities if his theory is to be worked out at all, just as the politician must act, if the whole machinery of the State is not to come to a standstill.

This is by no means meant to indicate that I now consider my idea of a regular dispersal of the germ-plasm in the course of ontogeny incorrect; it rather appears to me a more indispensable assumption than ever, but I recognise that liberation (Auslösung) plays a larger part in ontogeny than I had hitherto believed, and that the assumed breaking up of the germ-plasm does not, at least in many cases, take place at the first division of the ovum, but later.

It is not my intention to enter more fully into this question here, but I should like to emphasise the fact that a modification of my theory of ontogeny can in no way react upon the theory of heredity in the strict sense. My ideas as to the mechanism of heredity would remain unaltered even were I to adopt a pure liberation-theory such as

[1] "Address by the Most Hon. the Marquis of Salisbury," *Report Brit. Assoc.* Oxford, 1894.

has been recently developed by Oscar Hertwig.[1] I still believe as firmly as ever that the germ-plasm is made up of determinants,—that is, of groups of vital particles, the presence of which in the germ-plasm conditions the formation of definite parts of the organism, and the variation of which modifies these parts alone. The conception of the germ-plasm as composed of ids is also in no way affected by the newer facts, and it still seems to me not only a fruitful but an imperative hypothesis.

As far as the theory of heredity is concerned, it is a matter of indifference whether the determinants of the germ-plasm remain together during ontogeny, and are only individually excited to activity by specific stimuli, or whether they are dispersed during ontogeny into smaller and smaller groups. Even in the latter case liberating stimuli are also indispensable, and it does not affect the real problems of heredity—the inheritance of functional modification, the mingling of parental characters, reversion to nearer or more remote ancestors, and so on—in what particular way the individual determinants are excited to activity, provided that their existence is established. That they do exist, I consider certain, because it follows as a logical necessity from the facts of inheritance and variation. Determinants must be present because the parts of the organism are capable of individual and transmissible variation, and this is only possible if there are in the germ-plasm living particles related to definite parts of the perfect organism; they must be present, not in the sense of the "Anlagen" of the old "evolution" or preformation theory, in which they were regarded as being themselves the part in question although only *in nuce*, but in the sense of *working, living particles which operate during the course of development in a definite manner, so that the part they have to determine is thereby produced.*

It is only the theory of ontogeny which is affected by the fresh facts, not that of heredity. The experimental determination of the prospective importance of the blastomeres which we owe to Roux, Driesch, Wilson, Chabry, and so many others, and the now indubitable process of post-generation as formulated by Roux, necessitate the assumption that the blastomeres commonly contain all the determinants within themselves, and that the cells of the external and middle germinal layers retain many of them for a long time. But it by no means follows that a pure liberation-theory (Auslösungs-Theorie), like that put forward by Oscar Hertwig, is correct. It is much more probable that at some period of development, at least in the case of higher animals, a limitation in number of the "Anlagen" takes place in the cells during ontogeny, and this may often go so far that they eventually only contain the one "Anlage" which determines their differentiation. I cannot agree with the statement in Hertwig's latest book, in which the supposition is made that all the cells in ontogeny contain the complete

[1] "Die Zelle und die Gewebe" (II.), Jena, 1898.

germ-plasm, and that it depends solely on the stimuli of position and the nature of the particular cell-body itself, which of the many "Anlagen" of the nuclear substance will undergo development. I may, indeed, have laid too little emphasis on the rôle of the liberating stimuli, and bestowed my attention too exclusively on the "Anlagen," but such an extreme liberation-theory as that of Hertwig seems to me to overshoot the mark on the other side.

If what should be formed at a particular part of the organism depended solely on the liberating stimuli, and the "Anlagen" were everywhere the same, the same organ would necessarily invariably be formed at any given place; and it would be impossible that—as has many times been observed—an antenna should grow out in place of an insect's leg (Wheeler), or a leg instead of an antenna (Kraatz), or that instead of an abdominal appendage in the common crab (*Cancer pagurus*) a walking leg should appear (Bethe), or that antennae should replace the amputated eye-stalks in shrimps (Herbst), or that four-jointed tarsi should replace five-jointed ones (Bateson, Bordage).

In regard to a few of these cases, one might seek to evade the conclusion by the unverifiable statement that the liberating stimuli are altered by the previous occurrence of injury. This may be so, but still no explanation follows thereupon as to why, for instance, a four-jointed tarsus is formed in place of a five-jointed one, since the four-jointed tarsi of the ancestors did not result from amputation. I do not see how it is here possible to avoid the assumption of different "Anlagen" which can be liberated by a similar stimulus; and thus that the "Anlage" is the main thing and the stimulus only of more general nature; the stimulus only determining that something is to be formed, the "Anlage" deciding what that something is.

I hope to discuss this question more fully on some other occasion, and therefore here only wished to indicate my position in regard to some of the facts and opinions which have come to light since the publication of the "Germ-Plasm." It will probably be necessary to make a compromise between the theory of dispersal and that of liberation, though it may not yet be possible even to sketch its outlines with certainty.

With regard, however, to the theory of regeneration, which is so closely dependent on that of ontogeny, the explanation of the renewal of a lost part is easy enough if we regard the complete germ-plasm as present in every cell; every "Anlage" could then come into operation at any place if we assume their specific liberation-stimuli to be present. But the effectiveness of the theory breaks down when we attempt to explain why, nevertheless, regeneration in so many cases does not take place;—why, for instance, internal and external parts of the same newt should behave so differently in this respect. If the injury supplies the specific liberation-stimulus, why does it not liberate the corresponding "Anlagen" in internal parts also? There seems to be

only one possible answer to this question, viz. because the "Anlagen" are not there.

The facts recently established certainly indicate that, at any rate among many animals, the embryo and the young organism possess many cells whose wealth of "Anlagen" can, under the determining guidance of the whole, bring about the regeneration of parts in different directions; but it seems to me equally certain that, as the organism approaches maturity, the number of such cells gradually decreases, and the potentiality of their "Anlagen" becomes gradually more restricted, until, finally, many cells are only able to reproduce their like. But how far this limiting of the "Anlagen" goes, and how far the cell contains inactive "Anlagen" ("Neben-Determinanten"), is obviously determined by the necessities of the case, and depends on adaptation; in so far, at least, it will hardly be disputed that the capacity for the regeneration of parts is regulated by adaptation. It is impossible to say whether we must not go much farther than this, and regard the power of the blastomeres to produce the whole as a very ancient but nevertheless secondary phenomenon, depending on adaptation to injury. But if we bear in mind how the differentiation between homoplastids and heteroplastids occurs in the Volvocidae, it is evident that the first separation of the germ-cells and the body-cells in this case cannot be referred to different liberating stimuli, but must consist in a separation of the "Anlagen"; the latter must therefore be the primary arrangement, and the equipment of the blastomeres with the whole germ-plasm only a secondary acquisition. We thus once more arrive at the view, adopted both by Roux and myself, of "reserve germ-plasm"—of supplementary germ-plasm in general,—with which Barfurth also agrees.

To this important point, too, I hope to return at a future time, when I intend to reply to the factual criticisms of my opponents. This short essay owes its origin to purely external reasons, and I have written it rather for the supporters than for the opponents of my views, and rather to show that the recently established facts harmonise well with the fundamental principles of my theory than to meet the objections of my antagonists in detail. To reply to the many invectives, the sarcasm and derision which have been showered upon me in such overflowing measure since the publication of the "Germ-Plasm" seems to me quite superfluous, for I regard such utterances as a not exactly desired, but yet a not altogether unsatisfactory sign, that the less noble emotions of human nature—envy, ill-will, and self-advertisement—have found cause to direct themselves against the results of my work.

To one reproach of a general and impersonal kind I should like to reply here, as it was first raised by one who is now dead, but has been urged again and again by those who wished to bring discredit on my views—the reproach that these views have no stability and that they change so ceaselessly that no one can know what my real opinion is. As a matter of fact, my ideas have changed on many points in the

course of the score of years which have almost elapsed since I first brought them forward, simply because I did not then know all that I know now. I may be permitted to give an example.

In 1885 I directed the attention of biologists to the importance of the "polar bodies" in relation to the theory of heredity. The persistence with which these bodies, in themselves apparently insignificant, appear throughout the whole animal kingdom seemed to me to indicate that they played an important physiological rôle which must be associated with their liberation from the egg, and in connection with the theory of the continuity of the germ-plasm—then only developed in rough outline—I suggested that they might have to do with the extrusion of the "ovogenetic" idioplasm, that is, with the removal of the nuclear substance determining the histological structure of the egg-cell.

This supposition of mine—my first hypothesis concerning the importance of the polar bodies in relation to the theory of heredity—was opposed to the opinions expressed earlier by Minot and E. van Beneden, who regarded the immature ovum as a hermaphrodite cell which became female only after the extrusion of its male elements, the polar bodies. Observation showed me that even parthenogenetic eggs form polar bodies, and consequently Minot's hypothesis was overthrown, while my own was strengthened. But further investigations by Blochmann and myself led to the discovery that parthenogenetic eggs only form one polar body, while those which require fertilisation always form two; and then, supported by E. van Beneden's fundamental discovery of the numerical equality of the paternal and maternal chromosomes in the fertilised egg, I modified my first hypothesis so far as to suggest that the "ovogenetic" idioplasm is expelled in *one* of the polar bodies, while the other implies a *reduction of the ancestral germ-plasms*, or *ids* as I now call them. The necessity of this process I attempted to prove, and postulated for its accomplishment a special mode of nuclear division, a *reducing division*, in which the usual longitudinal splitting of the chromosomes is suppressed, so that only half the number of these migrate to each daughter-cell. What was at the time only a theoretical conclusion merely indicated by the observations then on record and not deducible from or capable of proof by them, has been fully justified by later investigations. There *is* a reducing division.

In this second hypothesis, however, an error was also involved, for Boveri's[1] observations on the egg of *Ascaris* have shown that the chromosomes expelled from the ovum in the first polar body are not essentially different from those remaining in it, because in certain circumstances they are interchangeable with them. I therefore altered my second hypothesis to a third, according to which both divisions of the ripening ovum together imply a reduction in the number of *ids*,

[1] "Zellenstudien. Über das Verhalten der chromatischen Kernsubstanz bei der Bildung der Richtungskörper und bei der Befruchtung," *Jenaische Zeitschr. f. Naturwiss.* 1890, Bd. xxiv. p. 314.

and this position I still maintain. Previously to this, however, I had come to the conclusion, based upon the theory of the reducing division, that a corresponding process of reduction must take place in the male cell, and some time later this was actually demonstrated by Oscar Hertwig. It is well known that during the succeeding years many excellent observers have carried on this difficult investigation on the processes of reduction in male and female germ-cells, and I need only mention the names of Boveri, Henking, vom Rath, Rückert, Häcker, Korschelt, Ischikawa, and van der Stricht, and in the domain of botany, Strasburger, Ischikawa, Calkins, Belajeff, and Guignard. Thus the conclusion has been gradually more and more firmly established that in the sex-cells of animals, as well as in those of plants, a reduction of the number of chromosomes takes place, and that in the majority of carefully investigated cases this process is brought about in the same manner, that is, by a peculiar form of nuclear division which occurs only in this connection. Recently, too, increasingly reliable evidence has been furnished that a similar reducing nuclear division precedes conjugation in unicellular plants and animals (Maupas, R. Hertwig, Schaudinn, and others).[1] Isolated observations are always cropping up which seem to necessitate different interpretations; but we may expect, as has often happened before, that more thorough investigations will succeed in removing the discrepancies in some way or other. A review of the facts now established cannot but convince us that for a large series of organisms the reduction-hypothesis is already proved.

This advance in our knowledge was only possible on the basis of *changing* theories, one growing out of another, and it would be absurd to object to such modifications. It seems to me, on the contrary, that the great value of hypotheses and theories is just that they do bring about the necessity of modifying them. They form the indispensable ladder by which investigation descends step by step into the depths of the biological mine until it comes upon a new vein of ore—fresh guiding facts—when it pauses a while, exploring and exploiting them in all directions, and finally by proving them secures a basis of operations from which a new ladder may be sunk to lower depths.

One of my critics has compared my "theories" to "towns in the Far West," the houses of which are barely erected when they are taken down again to be rebuilt farther out in the unknown land. I accept the simile, provided it be not forgotten that the first house of the advancing pioneer must remain standing and in use for a time before the region beyond becomes accessible to further colonisation.

I admit that I have not only made unavoidable mistakes, but also some which might perhaps have been avoided, and that I have

[1] Cf. the excellent review and critical summary of facts recently given by Häcker (*Verh. Deutsche zool. Gesellsch.* Jena, 1898) under the title "Über vorbereitende Theilungsvorgänge bei Thieren und Pflanzen," p. 95.

occasionally over-estimated the bearing of a newly acquired item of knowledge in the joy at its discovery,—that is, I have looked at it from too one-sided a point of view. This was the case in my paper on the meaning of sexual reproduction, in which I represented this factor in individual variation as its real and main root, while it is, I now believe, only the indispensable means of mingling these variations and of their persistent renewal. It is immaterial whether I myself or one of my sharp-sighted critics was the first to discover this mistake; but in any case it gave me occasion to sink a new ladder into the mine of investigation, to bore for the deeper ore-stratum of the source of variation, and to find it in the minute local fluctuations in nutrition which strive to disturb the equilibrium of the system of determinants in the germ-plasm. In this way I arrived at the theory of "germinal selection," which, although recognised only by a few—publicly, I believe, only by Emery—as a justifiable hypothesis, will, I should like to believe, yield greater results in the future.

It has also been said that I have carried my theories much too far, and have lost myself in details and speculations. Ironical admiration has been bestowed on the courage which enabled me to apply my theories to particular problems, and which did not shrink from the many auxiliary hypotheses necessitated by applying the main theories to special questions. To speak thus is to overlook the fact that nothing but persistent pushing of a guiding hypothesis to its furthest limits leads to a recognition of its defects and weaknesses, and so paves the way for farther progress. It is quite easy to set up theoretical principles, to propound a "theory of heredity" or of ontogeny, if one confines oneself to elucidating only the most general phenomena. The test of the usefulness of theories lies in applying them to special phenomena; it then becomes apparent whether and where they touch improbabilities, and where new facts are required to improve them or to replace them by others. It is not the joy of "explaining everything," but the desire to see them tested on all sides and to give them freely into the hands of my critics, that has led me to apply my principles of explanation to particular problems and to carry them as far as possible. This has been well understood by Yves Delage[1]—by far the most objective of my hostile critics—in the following passage: "Enfin il faut savoir gré à Weismann d'avoir été jusqu'au bout des conséquences logiques de son système. *Il a tenu à tout expliquer*, et il n'a pas reculé devant la nécessité de compliquer sa conception fondamentale, si simple et cependant déjà si féconde, pour rendre compte des faits de bourgeonnement, de régénération, de polymorphisme, etc. Il aurait, en évitant d'en parler, comme tant d'autres, échappé à de graves objections; il a préféré les subir que de reculer devant les difficultés."

If it be asked why I did not keep silence altogether if many of

[1] "La structure du protoplasma et les théories sur l'hérédité," etc., Paris, 1895, p. 708.

my explanations were not satisfactory even to myself, I can only give the simple reason that the fundamental basis of the theory appeared to me useful, and a fully worked-out theory seemed necessary to raise new questions and lead to further progress. In the complicated domain of biology, and particularly in that of heredity, it is only through theorising that new questions are raised, and the path to the discovery of new facts pointed out.

The assumption of the existence of biophors and determinants seems to me as indispensable and quite as justifiable here as the assumption of atoms and molecules in the domain of chemistry. Modern philosophy would, indeed, be in the right in rejecting the idea of philosophical atoms as minute indivisible particles; the chemical atom, on the other hand, is so far real, as it is the expression of the relative weights in which the so-called elements combine together to form molecules. Without the symbol of chemical atoms and molecules, the whole marvellous deepening of chemical knowledge which the century now drawing to its close has brought about would have been impossible. In the same way, I believe that a deeper penetration into the problems of biology, or at least into that of heredity, will only be possible through the assumption of the symbols of biophors, of ids, and above all of determinants; and here also, when, with the help of these symbols, we formulate our questions to Nature, we shall be dealing, not with mere fanciful images, but with realities, just in the same sense in which chemical atoms and molecules are realities.

In my opinion, the belief that biology does not require the guidance of theory is a great and widespread error. Within a very limited range, an individual investigator may, without doubt, make progress in certain directions without any conscious guiding theories—that is, so to speak, with latent theories. In truth, however, if his research be at all far-reaching, it is always guided by hypotheses, the confirmation or refutation of which he seeks. But when an attempt is made to investigate a wide domain embracing many phenomena, unconscious assumptions are no longer sufficient; but definite and well-thought-out hypotheses belonging to a theoretical system are necessary. This is what I have attempted to supply with my theory of heredity,—not a structure calculated for eternity, but one which should serve as a solid nucleus, a crystallisation-point, for further research, and around which new acquisitions in knowledge should group themselves for a time. I do not believe that any one now living could think out and elaborate a theory of heredity which should withstand the course of time without requiring modification. The scientific general-staff is now so large, that no hypothesis can remain long untested; on the contrary, every new idea is at once seized hold of by an army of investigators who seek, if possible, to refute it, or at least to corroborate it. I do not understand how it can be seriously urged at such a time, as an objection to a theory in the complicated

field of Nature and Life, that it has been modified according to new discoveries, which it has itself in part brought about. That it should be capable of such modification without giving up its fundamental principles seems to me an advantage, and I trust that in the future, even more than in the past, the theory of the germ-plasm will do good service in widening our knowledge of heredity.

ZOOLOGISCHES INSTITUT,
FREIBURG-I.-BR.

FRESH FACTS.

GEOTAXIS IN ANIMALS. JULIA B. PLATT. "On the Specific Gravity of *Spirostomum*, *Paramaecium*, and the Tadpole in relation to the Problem of Geotaxis," *Amer. Naturalist*, xxxiii. 1899, pp. 31-38. The tendency that some Infusorians have to collect near the surface of the water in which they live has been regarded as a reaction to the force of gravity,—a negative geotaxis. To test this Miss Platt has followed Dr. C. B. Davenport's suggestion of altering the density of the fluids. If F. Schwartz was right in his theory of negative geotaxis, the negatively geotactic organism should become positively geotactic in solutions of greater specific gravity than its own, supposing the animal to be normally heavier than water. Miss Platt determined the specific gravity of living *Spirostomum* and dead *Paramaecium*,—both about 1·017,—but got no positive results in regard to reaction to gravity as far as these Infusorians are concerned. "Small tadpoles that are negatively geotactic do not become positively geotactic when placed in solutions heavier than their own specific gravity, as one would expect were their upward motion in direct response to the action of gravity on the organism as a whole. These tadpoles show constant negative geotaxis in water, in a solution of their own specific gravity, and in heavier solutions."

FRESHWATER PROTOZOA. ROBERT LAUTERBORN. "Protozoen-Studien, IV. Theil. Flagellaten aus dem Gebiete des Oberrheins," *Zeitschr. wiss. Zool.* lxv. 1899, pp. 369-391, 2 pls. An impulse to the study of freshwater Protozoa should surely be found in the researches of Dr. Lauterborn, which show how little we know as yet of the numerous beautiful forms inhabiting ponds and lakes and river-beds. In a short time and within a limited area he discovered five new genera of Flagellata.

REGENERATION IN TUBIFEX. H. HAASE. "Ueber Regenerationsvorgänge bei *Tubifex rivulorum* Lam. mit besonderer Berücksichtigung des Darmkanals und Nervensystems," *Zeitschr. wiss. Zool.* lxv. 1898, pp. 211-256, 2 pls. 11 figs. There is less regenerative capacity in *Tubifex rivulorum* than in many related Annelids. The posterior part is regrown more readily than the anterior end, where in fact never more than three segments are replaced. Except a small most anterior portion the regenerated fore-gut is endodermic, and not ectodermic as in normal development. On the other hand, the hind-gut is regenerated from the ectoderm just as it is formed in the embryo. The regenerated ventral nerve cord arises from an unpaired proliferation, but the supra-oesophageal ganglion has a paired origin which is, however, to begin with ventral in position.

PARTHENOGENESIS IN STICK-INSECT. STADELMANN. "Ueber einen Fall von Parthenogenese bei *Bacillus rossius* F.," *SB. Ges. Natur. Freunde Berlin*, 1898, pp. 153-155. The occurrence of two successive parthenogenetic generations is recorded, thus confirming the previous observations of Dominique, Bolivar, and Krauss. The parthenogenetic ova developed into females only.

BIRD-CATCHING SPIDERS. R. I. POCOCK. "The Genus *Poecilotheria*: Its Habits, History, and Species," *Ann. Nat. Hist.* iii. 1899, pp. 82-96, 1 pl. This genus is a representative of that great and almost cosmopolitan group of spiders which was formerly included under the comprehensive title *Mygale*, which still persists in many books. The observations of Madame Merian and Mr. Bates have formed the slender foundation for the widespread and sensational belief that the staple article of food of these spiders consists of small birds. "As a matter of fact, there is no doubt that they feed almost entirely upon insects; but they will certainly also kill and devour any living animal they are powerful enough to overcome. The species of *Poecilotheria* are tree-living forms, and their colouring is adapted on the upper surface for protective concealment on bark overgrown with lichen and moss. The colour of the under side is startlingly different, is exposed when they rear themselves on their hind legs, and is probably of warning significance. They also possess a stridulating organ between the mandible and the palp. Eight species are described from Ceylon and S. India."

SELECTIVE ACTION OF FROST. T. D. A. COCKERELL. "Vernal Phenomena in the Arid Region," *Amer. Naturalist*, xxxiii. 1899, pp. 39-43. From observations on plants and bees made by himself and other workers at the Mesilla Park Experiment Station, New Mexico (altitude 3800 feet), Prof. Cockerell concludes that "throughout the arid region, where the sky is clear and the radiation great, the development of plants and insects is controlled largely by the distribution of frosts throughout the year. In the course of natural selection the native species (except such as are frost-proof) have learned not to appear or develop until the danger of frost is over." There is warmth enough, as is shown by the behaviour of introduced fruit-trees which rush into bloom and get nipped. The horticulturist's device must be to select late varieties which will escape the frosts.

DIMORPHISM. E. G. CONKLIN. "Environmental and Sexual Dimorphism in *Crepidula*," *Proc. Ac. Nat. Sci. Philadelphia*, 1898, pp. 435-444, 3 pls. In various species of this genus of sedentary gasteropods there is "environmental polymorphism," *i.e.* the form and colour differ according to the nature of the shells on which the animals live. In *Crepidula plana* there are dwarf and giant forms, and the difference in size is associated with a difference in the number, not in the size, of the component cells. The dwarfing is a "modification" and not inheritable. In the same species sexual dimorphism is also well marked, the average female being about fifteen times the size of the male.

DETECTION OF A ROBBER ANT. E. WASMANN. "*Lasius fuliginosus* als Raubameise," *Zool. Anzeig.* xxii. 1899, pp. 85-87. This glossy black wood-ant, which finds its usual food-supply in aphides, has not hitherto been known as a robber. But Wasmann has detected it in the act of plundering the nest of *Myrmica laevinodis* and carrying off the larvae and pupae. The observer does not believe, however, that we have here any hint as to the origin of a mixed colony. The captives are simply devoured.

SOME NEW BOOKS.

THE NATURALIST IN AUSTRALIA.

In the Australian Bush and on the Coast of the Coral Sea, being the Experiences and Observations of a Naturalist in Australia, New Guinea, and the Moluccas. By Dr. RICHARD SEMON. Roy. 8vo, pp. xvi. + 552, with 86 figures and 4 maps. London: Macmillan and Co., 1899. Price 21s. net.

It is right that this book, which was first published in Germany some three years ago (see our previous review of the original edition), should have been translated into English. It is professedly an account of an expedition destined to bring some phylogenetic problems nearer their solution; and as such it would be welcome to many English readers who are interested in the "living fossils" of the island continent. But it speaks to a wider class than those who trouble themselves with questions of affinities between the various groups of animals: every one with even a trace of the feelings of a naturalist and every one who concerns himself with the character and doings of primitive man is appealed to. Semon does not attempt to give details of the scientific results of his expedition, for these are appearing in his "Zoologische Forschungsreisen," but he gives a fascinating account of his observations of the habits of the animals that attracted him to the southern hemisphere, and he adds a lively sketch of the life and character both of the squatter and the "blackfellow."

The present writer has only recently returned from the region of Australia that is most fully described by Semon, and can heartily testify to the truthfulness of the book. Semon has succeeded in transferring to his pages much of the spirit of the bush; he gives no exaggerated account of life in the wilds, but, speaking with full appreciation of his facts, tells simply and happily the story of his adventures.

Much of the interest of such a work might have been lost in a translation, but such is not the case here; and the English edition is thoroughly readable. There are, it is true, a few obvious errors in the rendering, and a number of words and phrases with a distinctly foreign colouring; yet on the whole the version is most satisfactory.

The translation, as well as the method of the author, may be illustrated by the account that is given of the Australian opossum (*Trichosurus*). Semon figures this animal, and tells us about its skill in climbing and its nocturnal wanderings in search of insects, eggs, young birds, eucalyptus buds, and sour grapes. He describes the kind of trap with which he secured many specimens, and in connection with the observation that few of his victims had young in their uteri, he digresses to explain the bushman's belief that the young of marsupials are conceived on the teat. But when he comes to describe his experiences in shooting 'possums, he is even more discursive. Others in like position have felt the weird influence of the moon's rays shining through the

great gums, but surely no one has moralised on his feelings to such effect. "In Middle Europe," says Semon, "the moon during the summer months stands particularly low in the heavens, and remains but a short time visible. And still this is the very season when the balmy night air suffers us to stay out-of-doors to enjoy nocturnal scenery. In the tropics she stands nearly all night in the zenith just above us. . . . The towering gum-trees stand white and shining as if decked with silver, and almost without casting any shadow. Every line, every leaf, shows distinctly, still the penetrating clearness of day is wanting, and a mysterious veil lies over the shimmering landscape. Were I a poet I would try to depict the impression of this sight in the human mind. Being a naturalist, I feel the desire to analyse, and thus I ask myself: What is the reason of this wonderful effect, this magic and mysterious element in the moon-light landscape, which makes it an object of folk-lore and poet's fancy? The answer to this question is near at hand. The effect of moonshine owes its peculiar character to the circumstance of its giving light enough on clear nights to illuminate a surface directly exposed to its rays. Thus, for instance, an open book will be lighted up clearly enough to allow of your reading it. On the other hand, however, it is not powerful enough to call forth a perceptible brightness when diffused, *i.e. indirectly reflected* by the sky or the ground. To this diffuse light we owe, however, the possibility of recognising in daytime those parts of objects not directly exposed to the sun or even those that are situated in the deepest shade. The blackness of the shadows in the height of daytime is only a seeming one, brought about by contrast. . . . The contrary is the case with moonlight. Here the diffuse reflected light is so weak as to leave those parts of an object, which are not directly exposed to the rays, in darkness, and make their details disappear to our sight. The chief characteristic of moonlight thus consists less in the difference of its hue from that of the sun—though this, too, strikes us in a peculiar way—than in the absence of diffuse light and the almost absolute blackness of the shadows."

All through the book one finds facts made the basis of reflections; and this undoubtedly adds much to the pleasure of the reader. But even when Semon does not digress to philosophise about his facts, he manages to add to their interest by his enthusiasm. No one will read unsympathetically his account of his success with *Ceratodus*. For long he searched in vain for the eggs of this lung-fish; but one day Frank—the great good-for-nothing!—came running to the camp, "breathless and beaming with joy," thinking he had earned the reward of £1 promised to him who should first find the spawn. But Frank had not got the right thing, and it was about six weeks later that the "glorious discovery" was made. Semon tells of his troubles with the blacks, of difficulties in pickling the eggs and rearing the young fish: we mourn with him over some that were done to death by the excessive kindness of a girl-friend.

The later chapters of the book, relating to regions in which Semon stayed only a short time, are less satisfying than the earlier ones; but on the whole the work is to be recommended as full of interesting facts that are well told.

GREGG WILSON.

THE LIVES OF STREAMS

River Development, as illustrated by the Rivers of North America. By Professor I. C. RUSSELL. 8vo, pp. xvi. + 327, with 23 figures and maps. London: John Murray, 1898. Price 6s.

This is a very useful work on an interesting subject; yet there remains the feeling that it might have been better. A little more systematic arrangement might have been accomplished; there would have been avoided some repetition, and such a divorce as to separate 'the migration of divides' from 'stream conquest.'

River development is so complex a subject, requiring to be considered under so many headings, and the various phenomena so interact among themselves that the difficulty of a concise arrangement may be granted. Nevertheless that is just why it is important. Such a work seems to require division into, for instance, (1) river mechanics, (2) river growth, individually, (3) river life-history, throughout a geographical cycle, and (4) growth and decline of what may be called river groups.

Now, while the author has admirably considered the first of these divisions, he seems not to have made sufficiently clear distinctions in regard to the others. To indicate what is implied it seems necessary to turn to biological language. Thus our second suggested division may be called river ontogeny—the growth from source to sea; the third, river phylogeny—the phases of development during a geographical cycle; the fourth, geographical distribution—territorial expansion or contraction. For a river's capture of territory is a phenomenon capable of separate consideration from that of river phylogeny, though of course it has an important bearing thereon.

The author often speaks of the lives of streams. He implies thereby the life throughout the geographical cycle. He speaks of young streams to imply those existing at an early date of the cycle. But this seems to lead to a certain want of precision, because one is apt to think of young streams as those near the source—the head-waters—irrespective of the phase of development of the geographical cycle. And so with other ages. A little reflection will show the importance of making distinction between the growth of a stream itself, on its way to the sea, and the sequence of phases which it exhibits as the geographical cycle runs its course.

These criticisms are not intended as fault-finding. They are to point out how much may be gained by keeping the phases of river development distinct.

One fault, however, does run through this work. The author introduces a term "corrasion" (p. 29) to denote "the deepening and widening of a stream channel." Etymologically it is most unfortunate. For *corrado* means to scrape or rake together. It might suitably be applied to the process whereby a stream makes a gravel-bed; it cannot denote the processes by which a stream carries material away.

The author is not responsible for the terms composed of combinations of "sequent" which he uses. Yet these are unfortunate; because they are apt to clash with ordinary non-technical words, so that it is difficult to avoid saying, not of course in a sentence, but on a page, that an obsequent stream arises subsequent to and consequent on the growth of a subsequent, which is a subsequent event to a consequent. It reminds me of a child's question—"Is a donkey called a donkey because it is a donkey?"—a delightful confusion of concrete, abstract, and qualitative senses of a word.

Speaking of subsequent streams, the author does not fully discuss their development (p. 185), partly for the reason that he only takes an outcrop plain into consideration. In regard to obsequent streams he is certainly at fault. He says, in effect, that when a stream is beheaded a portion of the down side stream is reversed to make an obsequent. But the slope of the channel would not permit this. What does happen is the result of a slow process, of the growth of a new drainage, whereby an altogether new stream is started.

But though exception may be taken in such matters, the book may be thoroughly commended as one which puts before the reader a most interesting amount of detail concerning river development. Particularly may it win approbation for some charming photographs. Unfortunately the same cannot be said for the maps; they are not at all clear. S.

PAGE AND LAPWORTH.

Intermediate Text-Book of Geology. By CHARLES LAPWORTH, F.R.S., Professor of Geology, Mason University College, Birmingham. Founded on the Introductory Text-Book of Geology by the late DAVID PAGE, LL.D. 8vo, pp. xvi. + 414, with 174 figures. Edinburgh and London: William Blackwood and Sons, 1899. Price 5s.

Since the publication of the first edition of Page's "Introductory Text-book" in 1854, the twelve editions through which it has run have faithfully recorded the evolution of modern geology. The present excellent compendium of what is known on the subject may be viewed as the thirteenth edition, though so much expanded as justly to deserve Prof. Lapworth's new title. Part I. treating of Dynamical Geology, though brief, gives a fair summary of the external and internal agencies at work modifying the surface of the globe, special attention being directed to slow secular movements of the crust. In Part II. Petrological Geology, following upon a short chapter on aqueous rocks, a clear account is given of the igneous rocks, including a convenient working classification, while the chapters on the metamorphic rocks are particularly good, especially the résumé of the recent opinions on the various modes of formation of foliated rocks. In Part III. Historical Geology, after a general classification of the stratified rocks and a short chapter on palaeontology giving a modern classification of such animals and plants as are likely to occur in a fossil state, follows an account of each of the great formations as developed in the British Islands—their foreign equivalents being also indicated—and good figures of their characteristic fossils are given. As might be expected from Prof. Lapworth's experience among the older Palaeozoic rocks, the chapters on these formations are particularly strong, and the figures of the zonal forms of Cambrian trilobites and more especially of the graptolites of his Ordovician and Silurian systems form an especially valuable and new feature of the work. Another valuable new feature is the introduction of a series of geological maps of the British Isles, by means of which the student is made to realise how our country has been gradually built up around the axis of ancient crystalline rocks, the remains of which are now exposed in the North-West Highlands of Scotland and Ireland and a few small scattered inliers which peer through the newer formations of England and Wales.

The résumé given at the end of each chapter cannot fail to be of service to teachers. Altogether the book is excellent, and thoroughly recommends itself.

B. N. P.

STILL THEY COME.

A Text-Book of Botany. By J. M. LOWSON, M.A. 8vo, pp. viii. + 394, with 247 figures. London: W. B. Clive and Co., 1898. Price 6s. 6d.

This is a preparatory book for London University Intermediate and Preliminary Scientific Examinations. It is well illustrated, but it has not acknowledged diagrams taken from other sources. The subject-matter, as far as space admits, is up to date, but too much condensed for a beginner to follow intelligently. The opening chapters are presented as summaries of facts before the student has become acquainted with individual plants, hence they are of little educational value. Chapter IX., on the Structure of the Flower, or Chapter VII., on Nutrition and Growth, slightly modified, would have made a better beginning. In the hands of a private student the book may hinder rather than help; under the guidance of a judicious teacher the pupil may use the book with advantage.

R. T.

OBITUARIES.

WILLIAM RUTHERFORD, M.D., F.R.S.

BORN AT ANCRUM CRAIG, ROXBURGHSHIRE, APRIL 20, 1839;
DIED AT EDINBURGH, FEBRUARY 21, 1899.

THIS eminent physiologist was a graduate of Edinburgh University (1863). After a period of continental study he became assistant to Prof. Hughes Bennett (1865), and Professor of Physiology in King's College, London (1869). He occupied this Chair for five years, during the last three of which he was also Fullerian Professor of Physiology in the Royal Institution. From London he returned to Edinburgh in 1874, as successor to Prof. Hughes Bennett. His great gifts as a teacher and as an organiser raised the physiological department in Edinburgh to a position of proud prominence, and many now famous workers served their apprenticeship in his laboratories. His most important researches were concerned with the action of drugs on the secretion of bile, and with the structure of striped muscle; but his work as a professor was even greater. A lucid but dogmatic teacher, an eloquent lecturer, a skilful experimenter, a laborious student, a lover of music,—a remarkable personality, he will not soon be forgotten.

SIR JOHN STRUTHERS, M.D., LL.D.

BORN AT DUNFERMLINE, 1823: DIED AT EDINBURGH, FEBRUARY 24, 1899.

FROM 1841, when he began to study medicine in Edinburgh, till shortly before his death, John Struthers maintained an untiring industry. As Extra-mural Lecturer on Anatomy in Edinburgh (1847), as Professor of Anatomy in Aberdeen (1863-1889), as a member of the General Medical Council, as President of the Royal College of Surgeons, Edinburgh (1895-1897), as a student of the structure of Cetacea, he did what his hand found to do with all his might. He will be long remembered as one who helped to raise the standard of anatomical teaching, as a loyal servant of his University who contributed not a little to its advancement and extension, as a shrewd reformer of medical education, as an early exponent of evolution views in days when they were less popular than now, and as an anatomist with an infinite capacity for taking pains.

WILHELM BARNIM DAMES.

BORN JUNE 4, 1843; DIED DECEMBER 22, 1898.

BY the death of Prof. Dames Germany has lost one of its foremost palaeontologists. During the whole of his career, from 1871 onwards, he was associated with the University of Berlin, first as Custos in the Museum (1875), next as

Professor Extraordinarius (1878), and finally, on the death of Beyrich, as Professor Ordinarius (1891). He travelled extensively in Europe, and spent his vacations as far as possible in the study of stratigraphical geology. His University thesis (defended at Breslau, where he studied under Ferdinand Roemer) referred to the Devonian rocks of Silesia, and was published in 1868. Until 1881 most of his writings dealt with invertebrate fossils, and after that date he paid special attention to the Vertebrata. His best-known memoir is that on the Berlin *Archaeopteryx*, published in 1884. He also wrote important memoirs on *Plesiosaurus*, Tertiary Chelonians, Triassic fishes, and the extinct Cetacean *Zeuglodon*.

On January 18, Dr. KARL FRIEDRICH WILHELM CLAUS, Emeritus Professor of Zoology in the University of Vienna, well known for his text-books of zoology, and for his numerous researches on invertebrates. He was born at Kassel on January 2, 1835, studied at Göttingen and Giessen, and was professor at Göttingen and at Marburg before he went to Vienna.

CHARLES DRURY EDWARD FORTNUM, the mineralogist and antiquary, died on March 6, at Stanmore, in his eightieth year. Dr. Fortnum divided his collections of Australian insects between the British Museum and the Hope Collection at Oxford many years ago. In his later years he paid especial attention to majolica and gems, and was a liberal donor to the University of Oxford. He was a trustee of the British Museum.

On February 9, at the age of 81, Prof. KARL MÜLLER, till recently editor of *Die Natur*, of which he was one of the founders. The Rev. J. DIGGES LA TOUCHE, the geologist, at Stokesay in February of advanced age. He was the author of "A Handbook of the Geology of Shropshire," 1884; was President of the Caradoc Field Club; and made a geological model of the country around Stokesay. Prof. H. F. WÜSTENFELD, of Göttingen, who died at Hanover towards the end of February, at the age of 91, was well known for his "Geschichte der Arabischen Aerzte und Naturforscher." Dr. FRANCIS N. MACNAMARA, on March 5, at the age of 67, a prominent member of the Indian Medical Service, once Professor of Chemistry at the Calcutta Medical College. On March 13, from the plague, Major J. EVANS, Professor of Pathology in the Calcutta Medical College.

The death is also announced of the famous palaeontologist, Prof. CHARLES OTHNIEL MARSH of Yale. He was born in 1831, graduated in 1860, and became the first professor of palaeontology at Yale in 1866. An obituary notice will be published in our next number.

NEWS.

THE following appointments have recently been made:—Dr. R. T. Anderson, as instructor in histology and embryology in Harvard Medical School; Dr. Erich von Drygalski, as professor of geography at Tübingen; Dr. H. Held, as professor extraordinarius of anatomy at Leipzig; Dr. A. Y. Grevillius, to be assistant in botany in the Agricultural Experiment Station at Kempen on Rhine; A. W. Hill, as demonstrator of botany in Cambridge; Wladimir J. Palladin, as professor of botany in the newly established technical school in Warsaw; Prof. Fritz Regel of Jena, as professor of geography at Würzburg; Prof. W. F. R. Weldon, as Linacre professor of comparative anatomy at Oxford, in succession to Prof. E. Ray Lankester.

The resignations are announced of Dr. G. K. Niemann, professor of geography and ethnology in the Indian Institute at Delft, Holland, and of F. W. C. Areschoug, professor of botany in the University of Lund, Sweden.

Prof. Sir William Turner has been elected president of the British Association for the Bradford meeting.

Prof. E. Ray Lankester has been elected a correspondent of the Paris Academy of Sciences, and an associate of the Belgian Academy of Sciences.

The degree of LL.D. has been conferred by Aberdeen University on Mr. Chas. Stewart, F.R.S., curator of the Museum of Royal College of Surgeons, England; and on Mr. Geo. F. Stout, lately lecturer on comparative psychology at Aberdeen.

The degree of M.A. *honoris causa* has been conferred by the University of Cambridge on Dr. G. Sims Woodhead, the new professor of pathology.

Dr. Helmert, professor of geodesy in Berlin, has been elected a corresponding member of the Paris Academy of Sciences in the section of geography and navigation.

The statue of Helmholtz by Herter is completed, and will be placed in front of the University of Berlin, between those of the Humboldts.

The late Prof. W. Rutherford has left to Edinburgh University his valuable medical library and his collections of microscopical specimens and diagrams.

The annual statistical report of Glasgow University for 1898 shows a total of 1836 students, of whom 264 were women. The number in the faculty of Science was 117, as against 673 for Medicine, 762 for Arts.

The *Scientific American* records that the Indian Government has been offered by Mr. Jamestji Tata the sum of a million and a quarter dollars for the establishment of a university for research on the model of Johns Hopkins.

A site has been secured at Kemp Town, overlooking Queen's Park, Brighton, for the Gardens of the recently started Zoological Society of Brighton and Hove. The managing directors are the Earl of Llandaff and Mr. F. W. Frohawk.

On April 9, a monument to Pasteur will be unveiled at Lille, and a Pasteur Institute opened.

The *American Naturalist* reports that the University of Indiana will have its biological station this year at Warsaw, Ind., that a permanent station in connection with Tuft's College is proposed on the rich shores of Casco Bay, U.S.A., and that the Natural History Society of St. Petersburg has established a biological station on the shores of Lake Bologoy.

A magnificent laboratory for the application of the Röntgen rays, under the directorship of Dr. Mezquita, has been recently opened in Madrid.

According to the *American Naturalist* the department of scientific investigation of the United States Fish Commission has awakened to new life under the supervision of Prof. Bumpus, and it aims to aid science in every way not incompatible with the main business of fisheries. The laboratory at Woods Holl is to be kept open throughout the year, and students will be welcomed there at any time.

In presenting to Prof. Lapworth the Wollaston Medal, the President of the Geological Society of London, Mr. Whitaker, made use of the following words :— "I am glad to know that in an elaborate memoir on the Silurian Rocks of Scotland, shortly to be published, the officers of the Geological Survey do full justice to your series of papers on these rocks in the South of Scotland, saying that you have furnished a complete solution of the stratigraphical and palaeontological difficulties of the subject, and have discovered the key to the complicated stratigraphy of the region. The summary, at the end of the chapter on Previous Researches, is written to place clearly on record the distinctive features of Prof. Lapworth's achievements, . . . which resulted in establishing the true order of succession of the strata." These are words alike honourable to the Geological Survey and just to the leader of geological thought and teaching of our time.

At a meeting of the Royal Society on March 9, Mr. A. C. Seward read a paper on the structure and affinities of *Matonia*, an isolated type among existing ferns, on the whole approximating more closely to the Cyatheaceae than to any other family. The data furnished by an examination of the palaeontological evidence led to the conclusion that in *Matonia* we have a survival of a family, now represented by two species in a few localities in Borneo and the Malay Peninsula, but widely distributed in the Mesozoic epoch, and especially abundant in the European area.

At a meeting of the Royal Society of Edinburgh on March 20, communications were read by Sir John Murray on the temperature over the floor of the ocean and the maximum and minimum temperature of the surface ; and by Prof. J. Cossar Ewart on intercrossing and variation.

At a meeting of the Geological Society of London, on March 8, Dr. Arthur Rowe read a paper on the genus *Micraster*. The author showed that definite features in the test were confined to definite zones in the white chalk, and thus supplied a valuable key to the determination of age of this deposit, in cliff or inland sections. Dr. Rowe promised a further paper on the zones in the coast sections of the white chalk, based on a study of these echinoderms, a paper which should prove of considerable interest and value to professional and unprofessional geologists.

As retiring president of the Biological Society of Washington, Mr. L. O. Howard gave an address on the economic status of insects, which is printed in *Science* (1899, ix. pp. 233-247). The *pros* are six and the *cons* five, as stated in this address which is full of entertainment, *e.g.* the reference to the prophet Joel as "an agricultural pessimist," or the discussion of Isaac Weld's note that General Washington told him that the mosquitoes of Skenesborough "used to bite through the thickest boot."

At the meeting of the Hull Scientific and Field Naturalists' Club on Wednesday, March 8, Mr. J. A. Ridgway of Beverley gave a lecture on "Shooting Stars," describing *inter alia* the famous shower of November 1866, which he witnessed. At a sectional meeting held the previous Wednesday evening, Mr. D. Murray of Kilnsea, near Spurn, sent for exhibition the skull of an elephant which had been obtained from the beach—a curious fragment of jetsam, doubtless admitting of a prosaic enough explanation.

Mr. G. Abbott, honorary general secretary of the S.E. Union of Scientific Societies, asks us to notify that the date of the next congress, which will be held at Rochester, has been altered to May 25–27.

On February 18, Prof. L. C. Miall lectured at the Royal Institution on "A Yorkshire Moor," and his lecture—a charming study in natural history—is now printed.

At the Congrès des Sociétés Savantes to be held at Toulouse, April 4 to 8, there will be an exhibition of timekeepers, instruments, etc., divided according to the decimal system. The Geographical Society of Toulouse invites those interested in this reform to send instruments, diagrams, and published papers.

Among the prizes offered by the Reale Istituto Lombardo are the Fossati prizes for 1899 on the macroscopic or microscopic structure of the nervous system, for 1900 on the regeneration of peripheral nerve-fibres in vertebrates, for 1901 on the structure of the brain in higher animals.

A prize of 500 francs is offered by the Société de physique et d'histoire naturelle de Genève for the best monograph on a genus or family of plants. The memoirs, which may be in English, must be sent to Mr. Pictet, the president of the society, before January 15, 1900.

The Belgian Royal Academy has awarded prizes to Georges Clautriau, of Brussels, for his work on digestion in carnivorous plants, and to L. Cuénot, of Nancy, for his work on the excretory organs of molluscs.

At a meeting of the Edinburgh Geological Society, on March 16, communications were read on "Contact metamorphism round the granite of the Cheviots," by Mr. Herbert Kynaston; "The granite intrusions and allied rocks in the neighbourhood of Braemar," by Mr. E. H. Cunningham-Craig. Dr. Hepburn and Mr. Simpson described antlers of red deer and other remains from the superficial deposits of Hailes.

The Joint Committee of the Royal Society and the Royal Geographical Society, appointed to promote a National Antarctic Expedition, made application some time ago to the Council of the Royal Society and the Council of the British Association for grants of money in aid of the proposed expedition. The treasurer of the Royal Society has applied on behalf of the Council to the Government Grant Committee for a grant of £1000, and the Council of the British Association will recommend to the next meeting of the general committee that a like sum be contributed by the Association. There is every reason to believe that these grants will be made.

The president of the Royal Geographical Society, Sir Clements Markham, has received a letter from Baron von Richthofen, president of the Berlin Geographical Society, in which he refers to a meeting recently held in Berlin in connection with the German Antarctic Expedition. Addresses were delivered by Baron von Richthofen, Dr. Drygalski, who will be leader of the expedition, Count D'Arenberg, president of the Colonial Society, and others, and the greatest enthusiasm was displayed by the audience of some 1500 people. As did the speakers at the meeting, so Baron von Richthofen in his letter refers in very explicit terms to the expected co-operation of England in the great work of a thorough exploration of the unknown Antarctic area. The German Committee have laid down the different routes which the two expeditions might follow, in order that the joint work may be as effective as possible, while each expedition would be entirely independent of the other so far as internal organization and command are concerned. "There cannot be any doubt," says the *Times*, "that if the two expeditions, adequately equipped, carried out their work on a common plan, the gains to science would be immense, much greater than if the expeditions carried on their work entirely independently of each other. Moreover, from other points of view, it is desirable that two great and friendly nations like Germany and England should co-operate in promoting common interests whenever it is possible. It would therefore be in the highest degree humiliating if

Baron von Richthofen had to be informed that England cannot co-operate with Germany in this great work because the British Government decline to support an expedition, and wealthy Englishmen are not public-spirited enough to subscribe the few thousands necessary."

We are glad to learn that the Millport Marine Station, one of the youngest of British Marine Biological Stations, bids fair to attain the success which has fallen to some of its older rivals. During the past year the station has been in full working order, and the fact that the tables in the laboratory were occupied by working naturalists for varying periods on thirty-eight different occasions, shows that the station is both needed and appreciated. The committee must also feel much gratified at the interest shown in the Robertson Museum, which was visited by over 8000 persons last year. Many additions have been made to the station during the past year, but the Committee appeal for more funds to enable them to effect further improvements, and also for donations to the library of books or papers on marine zoology.

Prof. Carl Chun writes to Sir John Murray (*Scot. Geogr. Magazine*, 1899, xv. p. 143) reporting on the course of the German Deep-Sea Expedition after leaving Cape Town. They re-discovered Bouvet Island so often searched for in vain—apparently a volcanic cone entirely covered with glacier ice. Especially remarkable is the series of soundings taken on the edge of the pack from Bouvet Island to the neighbourhood of Enderby Land. A depth of over 3000 fathoms was frequently sounded, showing that the Antarctic Sea is much deeper than has hitherto been supposed.

The Socotra Scientific Expedition, led by W. R. Ogilvie Grant and H. O. Forbes, has returned to London after a very successful exploration of the island. The collections made include several remarkable forms, especially some beautiful butterflies. A wild donkey, said to be a species new to science, has been secured for the Zoological Gardens. A great part of the interior of Socotra has been surveyed, and correct geographical data obtained for a revised map.

The expedition despatched by the Imperial Academy of Sciences of Vienna arrived at Socotra shortly before the departure of the British party.

The Russian Geographical Society, with the aid of funds supplied by the Tsar, has organised a new expedition for the exploration of Central Asia. It will leave St. Petersburg this month under command of Lieutenant Koslow, and will probably occupy two years.

Mr. John S. Budgett, sent out to the Gambia by the Zoological Society of London, is meeting with success in his collecting, among his captures being large specimens of *Polypterus*.

The *Scientific American* announces that S. H. Cavendish, explorer, and Edward Dodson, the naturalist, "have gone to Patagonia to search for the giant ground-sloth, or megatherium."

A grant of £300 from the Works Travelling Scholars Fund, Cambridge, has been made to Mr. Skeat in aid of his scientific expedition to the Malay Peninsula.

Dr. K. Kishinouye, of the Imperial Japanese Fishery Bureau, arrived at Tokio on March 15. Since leaving England in August 1898, his work has taken him to France, Germany, Norway, Sweden, Finland, Russia, and Italy.

In a Chicago school, in which at least a thousand children are taught, experiments will be made with an "ergograph" for measuring mental endurance, the object being to determine the "fatigue period" when school work should not be pressed. We are not informed as to the nature of the ergograph, but it is said to be "adapted for the purpose."

The Association of American Anatomists has accepted the proposal of the editors that the *Journal of Anatomy and Physiology* should be the official organ of the Association, and Prof. G. S. Huntington, of Columbia University, has been nominated American editor.